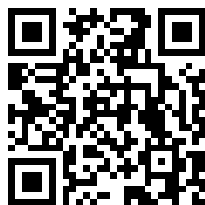

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A QUARTERLY REVIEW OF SCIENTIFIC THOUGHT

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APRIL, 1913.

VOL. 2

JAPANESE COLONIAL METHODS

By Ellen Churchill Semple

WHEN Japan does anything, the world looks on in an attitude of respectful attention. For Japan has given even the Western world a lesson in efficient methods and effectual results. It is therefore with some hesitation that we venture to chirp a criticism of her colonial methods, and that on points fundamental to successful colonisation. We frankly admit that those methods are on the surface highly scientific—too scientific; that they are carefully elaborated and faithfully applied; that they are animated by an intelligent and beneficent spirit to protect Japan's new subjects and to develop the resources of the newly acquired lands, with economy both to those lands and to the home Government. But when all this has been said, the facts still justify the question whether Japan's colonial policy is not calculated to defeat the great national purpose which should underlie all colonisation schemes.

Emerson says: "That which each can do best none but his Maker can teach him." This is eminently true of colonials.* These builders of empire act best on individual initiative. In them the *laissez-faire* policy finds ample justification. But Japan's policy makes no allowance for certain natural forces which see farther into the future of national development than the most intelligent Governments. Indeed, it has the colonising instinct of its people by the throat.

Political expansion has since 1895 acquired for Japan about 110,000 square miles of territory, into which ethnic expansion should follow. The new territories are not, according to Japanese

* The word *colonials* seems to me preferable to *colonists* because it is more comprehensive—includes not merely the original road-breaking colonists but their descendants living under colonial conditions.

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standards, densely populated, even in the cultivated districts, and they contain vast tracts of forest and waste land. Moreover, with the exception of Karafuto, the Japanese half of the island of Sakhalin, they are well suited in point of climate for Japanese colonists. Korea has a climate superior to that of Japan proper, and Formosa, though bisected by the Tropic of Cancer, in its capital Taihoku shares the mean annual isotherm of 70° F. with New Orleans and Northern Florida. Colonists from the sub-tropical islands of Shikoku, Kui-shiu and Liuchiu, where population ranges from 455 to 530 to the square mile, would find a tolerable climate and desirable space in Formosa, where they could pursue familiar lines of agriculture. Yet emigration from the home islands to these natural fields of expansion has almost from the start been checked or retarded by the Government's colonial methods.

The cause is not to be sought in any dearth of surplus population, that raw material of colonies. Hardly another country needs so much untrammelled colonial expansion to relieve the pressure upon its local supply of food and land. When we say that a population of 51,591,400 souls live on 147,655 square miles constituting Japan proper, and that they show a density of 350 to the square mile or a little less than that of the United Kingdom (373 to the square mile), this statement fails to reveal the most important facts. Geographic conditions make Japan a typical stepmother, doling out food to her children with niggardly hand. Two-thirds of its area, small at best, is covered with rugged mountains unfit for agriculture and useless for pastures, because the native bamboo grass, which is not only innutritious but also deleterious even to sheep, chokes out all imported fodder crops. Hence about 72 per cent. of Japan's area is in forests. The coastal plains are narrow, and the alluvial valleys are scant. Considerable tracts of the lowland hem of the islands are desolate rock wastes, owing to the detritus brought down by inundating mountain torrents in the rainy season. The sharp relief of the country and the heavy precipitation during the summer monsoons combine to make the rivers carve out narrow V-shaped valleys that offer a slender foothold to agriculture.

The effective area of Japan for food purposes is further limited

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by the infertile character of the soils. The volcanic nature of the country has been a disadvantage. Widespread ash-rains from the late Quaternary period have overlaid and ruined otherwise good land. The volcanic soils are poor or mediocre, except certain limited districts of weathered lavas and basalt which are the most fruitful in the islands. Granitic soils, poor in plant food, cover a large area; but since they readily absorb fertilisers, they are made productive by the laborious tillage of the Japanese peasant. Only the climate of Japan, with its warm sun, its abundant and well-timed rains, and its freedom from untimely frost, is the reliable ally of the toiling farmer.

With the means at his disposal the Japanese farmer has done his utmost. Geographic conditions in islands usually apply the spur to agriculture. Tillage begins early to assume an intensive scientific character, in order to feed an increasing population from a land area that cannot be increased. The inelastic, sea-drawn boundaries of the Japan archipelago have resulted in a precocious development of agriculture, and have given it an economic, national and æsthetic importance hardly to be found elsewhere. The farmer takes a high rank in the social scale. Though ignorant of scientific reasons, he has worked out a system of tillage that is wonderfully effective. He has succeeded in making a naturally infertile soil highly productive by intensive labour,—by deep-down cultivation, abundant irrigation, and especially by repeated manuring during the development of the crop. Practically without cattle and stock to furnish manures, he relies chiefly on night-soil as the obvious substitute in these densely populated islands. Long experience has taught him how to treat this in order to get the best results; to apply it to the soil at the moment when the constituent elements are chemically most available for plant food, and before the compounds have lost ammonia by excessive decomposition. Hence his practice long ago anticipated the laboratory results of European scientific theory. Fallow fields have no part in his system; his tillable land is too small. Intensive culture, dense population, high percentage of the farming class, and small arable area in Japan have combined to produce minute land-holdings. Dwarf farming prevails. Apart from the colonial country of Yezo, the chill northern island, 1 hectare or 2½ acres is the average

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farm per family. In 1908 over half the peasants tilled less than 2 acres, while only 1 per cent. of them held more than 12 acres.

Owing to disadvantages of relief and soil, arable land forms to-day only 14·37 per cent of the total area of Japan proper. Ten years ago it formed 13·7 per cent., and in 1887 it was 11·8 per cent. ; so the extension of the arable area proceeds slowly, despite growing pressure of population, progress of science, and encouragement on the part of the Government. The inference is that it has probably reached its limit. This small percentage of arable area means that the Japanese population of nearly 52,000,000, over 60 per cent. of whom are farmers, lives chiefly from the products of 21,218 square miles of its territory. A fact like this puts a new aspect upon the density statistics of Japan. Italy has a population of 314 to the square mile, as opposed to Japan's 350 ; but Italy has 49 per cent. of her area devoted to agriculture. Japan's density is nearly double that of fertile and thrifty France, with 55 per cent. under tillage ; and it is triple that of Greece, whose 18·4 per cent. of arable area shows a proportion comparable to that of ill-favoured Japan, but is reinforced by extensive pasture lands. Switzerland's density is only two-thirds that of Japan ; but the Alpine State has the 16·1 per cent. of its area which is under tillage supplemented by the 36 per cent. used for hay meadows and highland pastures. This advantage is, however, balanced by Japan's maritime location and abundant fisheries—those pastures of the sea.

Active emigration from all these European countries except France indicates that their respective territories, under existing economic methods of production, are saturated with population. Japan, with a greater nominal density and a vastly greater actual density, as well as a larger population to draw from, finds its emigration since 1908 narrowly restricted by Government measures, especially emigration to the United States, Canada, Mexico, and Hawaii. Count Komura, Minister of Foreign Affairs in 1909, explained this policy of the Government as a purpose to avoid all international friction which might militate against Japan's growing international trade ; and furthermore to keep the Japanese in the Far East, instead of allowing them to scatter at random in foreign lands, thus concentrating them in the extended fields of activity recently opened up by two successful wars. A natural inference

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from this statement is that Japan regards its redundant population as useful colonial material, and is prepared to make the most of its colonial opportunities ; yet, as a matter of fact, the natural outlet into these new colonial lands has been blocked.

Recent developments in Japan have created an urgent need for such an outlet. From time immemorial the farmer class has constituted the main productive power of Japan. Agriculture is still the chief basis of the national finance as in a mediæval State, but it now sustains a crushing burden of taxation, because Japan has launched upon the expensive career of a modern world power. The infant industries and commerce need the coddling of light taxation. Farms must pay from 15 to 19 per cent. of the total yield as land tax alone, besides certain other local rates. Dwarf holdings can ill afford such a drain. Hence peasant proprietors are disappearing at an alarming rate ; they are forced to mortgage their farms, which soon are absorbed into the larger estates and let out to tenants. In 1908 tenants tilled one-half the paddy-field area and 40 per cent. of the upland farms. Since they must pay from 45 to 57 per cent. of the total yield as rent, only by employing the whole labour force of the family, by maintaining a low standard of living, and often by combining with tillage some subsidiary occupation, can they earn a precarious subsistence. As the taxes of the Japanese farmer have increased, his revenues have diminished, because of foreign competition. The tea of Japan finds its market progressively restricted by the growing production in India and Ceylon ; its beans are being crowded to the wall by the great Manchurian export from Dairen, while its indigo, cotton and hemp crops have almost ceased to be remunerative. Only large-scale production can stand the large-scale competition of the world markets. Dwarf farms in Japan must go. The dislodged cultivator, if he remains a farmer, must seek the larger fields of Japan's colonial lands, there to practice modern large-scale tillage.

Japan has long had a genuine colonial or frontier region in the northern island of Yezo or Hokkaido (36,000 square miles), whose land reserves present on a small scale a parallel to those in the western part of the United States and Canada, though both soil and climate leave something to be desired. Its population in 1908 was only 30 to the square mile, after a steady stream of emigration from old

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Japan since 1896 had brought in about 700,000 souls or two-thirds of its present population. Of the 80,578 Japanese immigrants to the Hokkaido in 1908, nearly half came from North Hondo, a province only half so densely settled as the central and western provinces of the same island. But the relatively remote and segregated location of North Hondo, on the far side of the central mountain barrier of the island, had made it the earlier colonial district of old Japan. These northern Japanese, inured to a relatively harsh climate, accustomed to the tillage methods of the north, and divided by only a narrow strait from Hokkaido, were more easily attracted than the southern Japanese by the large tracts of available land and its teeming fishing grounds.

Hokkaido bears to-day all the marks of a typical frontier or colonial district. It is distinguished by the large allowance of 7.6 acres of tillage land for each farmer household, as opposed to 2.54 acres in Hondo. It is furthermore the important stock-raising district of the Empire, both on account of its abundant land and its suitability for grass crops. The island, according to official estimate, can support at least five times its present population. The State lands disposed of during the thirty-six years prior to 1907 amounted to 3½ million acres, and as much more remains for purchase or lease, by settlers. The terms are easy, both as to price, which is nominal, and to the planting and building to be done within a period of years upon the land purchased or leased. An individual may buy tillage land to the amount of 1,250 acres, forest and stock-farming land up to 2,000 acres. Small settlers may get 25 acres of land gratuitously on certain conditions; when they have satisfactorily finished work for the land acquired, they may purchase or lease more within the same limit. These are really munificent terms for Japan, a country used to dwarf farms, trained to the small territorial scale imposed by the cramped environment of the original islands.

Here we have genuine internal colonisation. The large number of women among the immigrants point to permanent settlements and orderly households. About two-thirds of the annual accessions to the population of Hokkaido belong to the farmer class. Abundant land, acquired under the easy conditions appropriate to a colonial territory, is an attraction strong enough to outweigh the rather forbidding climate. Hokkaido, exposed to the wash of polar currents,

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has even in its southern half a mean annual temperature of barely 45° F. Its growing season is limited to four months, when the temperature averages only 63° F. The farmer of central or southern Japan, who is accustomed to count upon seven or eight growing months, with a mean temperature ranging from 70° to 76° F. in the hot season, needs a strong inducement to face the bleak climate of Hokkaido. These inducements the Government has wisely supplied. To the over-taxed Japanese farmer especially grateful is the exemption from taxation. In 1909 some 1,650,000 acres of allotted land in the island were free of taxes for a term of years. In Hokkaido Japan has demonstrated her understanding of a genuine colonial policy, which aims to make solid ethnic expansion follow and overtake the previous political expansion. But her failure to apply the principle elsewhere in her new subject lands arouses the suspicion that it was chiefly an adverse climate, seconded by the obvious impossibility of otherwise developing this remote northern island, that forced Japan to this large colonial policy.

Her previous history had not trained her for such a policy. At this difficult trade she had served no apprenticeship, contrary to the experience of most island nations. The anachronism in her history was the policy of seclusion, adopted in 1624 and rigidly maintained for almost 250 years, by which all emigration and foreign trade were prohibited. By the more obvious laws of geographic probability, seventeenth-century Japan, like ancient Crete and modern England, should have colonised broadly and carried on an active maritime trade with her neighbours. Her small area, limited food supply, and sea-drawn boundaries, seconded by the possibilities of her tea and silk trade, should have forced her into commerce to feed her growing population, especially since in previous centuries she had reaped the benefit of an Asiatic trade, reaching from the mouth of the Amoor River to India, and had, moreover, developed the merchant marine wherewith to conduct it. This was the immediate effect of her maritime accessibility, favoured by her insular location close to Asia on the rim of a marginal sea, of her indented shore line, her well-populated coasts, and her sea-bred nautical efficiency.

But in 1624 Jesuit intrigue to overthrow the Government, and possibly to hand it over to Catholic Portuguese rule, threatened Japan's political integrity and gave sudden alarm to that jealousy

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of outside interference, that ineradicable instinct for aloofness, which had been bred in the people by their segregating island environment. Japan adopted and maintained for so long a policy of seclusion because her insular location both suggested and facilitated it. But she submitted to the isolating effect of that location up to the danger-point. Small, naturally-defined regions protect a nascent civilisation from outside attack and help to give it a definite aim by counteracting the primitive tendency towards dispersal ; they force it to exploit the group of geographic conditions and the economic resources of the home area without wandering far afield searching for lines of less resistance ; but in doing this they run the risk of teaching too well their lesson of concentration. In course of time geographic enclosure, like that of mediæval Japan, begins to betray its limitations.

The size of a people's territory influences their estimate of area *per se* ; it tends to fix the proportions of their territorial expansion and the scale of their private land holdings. A farm in Argentina or a ranch in western America would be a principality in Greece or Italy. A people embedded for centuries in a small natural district tend to measure area with a pocket foot-rule : the surveyor's chain is too big for their diminutive national purposes or their dwarf farms. This is the striking psychological effect of a narrow local environment. Therefore a vital turning-point is reached in the history of any people bred in such a habitat, when it breaks away from the clutch of its confined environment, and from a small, self-dependent community launches upon a career of conquest, colonisation, or any form of wide territorial expansion. This is the significance of England's vast outreach from the sixteenth century onward. There is no more telling fact in the history of the English in America than the rapid evolution of their spacial ideals, their abandonment of the cramped territorial conception brought with them from their island home and embodied, for example, in that petty land grant, fifty by a hundred miles in extent, of the first Virginia charter in 1606. The next colonial grants were bounded by parallels of latitude and the setting sun ! Cosmic boundaries are not too big for the colonial standpoint. Every accession of territory to the Thirteen Colonies gave a new impulse to growth. Expansion kept pace with opportunity. Only in small

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and isolated New England did the contracted provincial point of view persist. It was sloughed off soonest in the broad tidewater plains of Virginia. There colonists who learned the economic value of "skimp farming" under the tutelage of slave labour, and therefore took up large tracts of land for their plantations, lent themselves best to the national purpose of expansion.

Japan reached a great turning-point when she was stirred out of her apparent insular complacency by the Russian advance, and entered upon a career of expansion. Since 1895, when her new territorial policy was inaugurated by the acquisition of Formosa, her history reminds one of those explosive seed-pods which at maturity suddenly burst at a touch and scatter their seed abroad. She has acquired in ten years a colonial area nearly equal to that of the home archipelago, and she holds Manchuria with a grip that she does not expect to relax.

Such political expansion would seem to indicate a rapid growth of Japan's ideal of territorial aggrandisement. Very true; but unfortunately the ideal is only skin-deep. It has not permeated the economic and national purposes of the Government, to vitalise and stimulate them; and the Government, still suffering from a mental cramp, is checking the natural and healthy colonising instincts of the people. Meanwhile, the big colonial territory, in which Japan's large and vigorous surplus population might so easily realise a great national purpose of planting a new Japan on the new soil, is being administered by the Mikado's Government as it if were a fresh suburban addition to a growing town, exploited according to the methods of the average real-estate agent. While the authorities, with the national genius for administration, are organising schools, courts, sanitation, reforestation, railroads, frontier defences, and surveys of the vast Government lands in the new dependencies, to the emigrant from the home islands they offer no opening in the big task of colonial development. They bestow elaborate attention upon the newly-built hive, but drive off the swarming bees.

Let us consider first Karafuto, the Japanese holding in Sakhalin, as presenting the simplest conditions. Karafuto contains about 12,000 square miles of territory, unoccupied at the time of its acquisition except for 300 Russians who were too poor to leave,

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and about 2,000 aborigines. Handicapped by the same zonal location and climate as Newfoundland, it is a typical penal island. As a colonial proposition it would require a sugar-coating process to make it go down. The sugar is there in the form of abundant State-owned lands and excellent fisheries, to be applied with a generous hand. What do we find? Japanese to the number of 61,800 crossed over to the island in 1907 and 1908; one-fourth of these were females, a proportion that suggests a body of permanent settlers. But emigration must have been almost as active as immigration, for at the end of 1908 Karafuto contained only 23,139 Japanese. About a thousand families of these (4,000 souls), "in compliance with special inducements offered by the Island authorities"—we quote the official report—settled on the land deserted by the Russians, at the rate of about 5 acres per family. This allowance of tillage land is far more niggardly than the average of $2\frac{1}{2}$ acres constituting the dwarf farm of temperate and sub-tropical Hondo, considering the harsh climate, short growing season, and less profitable crops (oats, rye, beans, and tubers), in Sakhalin, whose mean annual temperature is only 41° F. Though the plains of Karafuto, according to official estimate, contain 112,500 acres of tillage land and 137,500 acres suitable for pasturage, and though practically all of this is State-owned, it is measured out to the settler on a cramped, foot-rule scale disastrous to colonial development. The peasant on his poor 5-acre farm in Karafuto finds nothing to compensate him for exile from warm and beautiful Japan, so he does not remain.

The close adherence of settlements to the sea-coast of Karafuto suggests that the colonial ekes out his crops by fishing. Fisheries are the important resource of the island, but they too are administered in an uncolonial manner. Nowhere are they free. Seine fishing is permitted only on special grounds and by special licence issued for a certain number of years. Ordinary licences are issued for 1,300 distinct fishing grounds, the rights to which are sold at public auction. On other grounds fishing is restricted, and on others again it is absolutely forbidden. These regulations may answer some purpose to conserve or distribute the fisheries, or to exploit them for the national revenue; but what is good for fish or finance may prove destructive to a large colonial policy,—to

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the colonial spirit of initiative, of untrammelled enterprise, of desire to do things on a big scale, and to carry them through with a rush and a shout in a manner which is called American, but which is simply colonial.

The land policy of the new Government in Korea reveals the same foot-rule standard, imported from the narrow and crowded conditions of the home islands. The density of the population in Korea (according to the last census) is only 154 to the square mile, as opposed to 350 in Japan. The tilled land constitutes only 10 per cent. of the whole peninsula, or half the area that might readily be brought under cultivation. It is estimated that the country by an extension of its agriculture could support an additional population of seven millions. Korean soil consists largely of a light sandy loam, disintegrated lava, and a rich, stoneless alluvium often many feet deep. The rainfall is well-timed and adequate; the country rarely knows droughts or floods. Ample facilities for irrigation and long warm summers afford favourable conditions for the all-important rice culture. Korea has an area of about 84,000 square miles and a population of nearly thirteen millions; but it can undoubtedly support in addition no small part of the surplus population of Japan. Immediately after the Chinese war, in 1895, there was an exodus from the home islands to the peninsula. Yet at the beginning of 1910 only about 145,000 Japanese were settled in Korea. What has checked the rush?

There is no dearth of public lands. The Government owns 319,800 acres of farm lands which is let to tenants, and in addition about 3,000,000 acres of waste land, which is susceptible of cultivation. In order to develop the waste tracts, the Government resorted to leases, a system advisable in colonial countries only as an adjunct to free grants of lands, and those generous ones. A law passed in July, 1907, provided for the lease to any person of State-owned waste land at a nominal rental for a term not exceeding ten years, for tillage, stock-breeding, or afforestation. Up to January 1st, 1911, since the passage of this law, 925 Japanese applied for a total of 205,000 acres. Only eighty-three of the applications were honoured, and they received a total of 9,380 acres. Koreans, at the same time, to the number of 1,176 made applications for the lease of 183,600 acres, but only 133, or about

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11 per cent. of them, received 14,900 acres, or a *per capita* allowance of 112 acres on lease. But during this period of three and a half years certain previous leases to twelve Japanese and thirty-seven Koreans, covering 13,440 acres, were disallowed and the land recovered, so that only 10,840 acres were thrown open to settlement for 167 lessees. This system, as put into practice, therefore offers scant encouragement to colonists ; it is rather a methodical rebuff.

A letter to the writer from the Secretary of the Governor-General of Korea assigns various reasons for this reluctant application of the ten-year lease system. In some cases the tract applied for had to be preserved for the future development of the whole locality ; or its use would entail hardship upon neighbouring fields because of insufficient water for irrigation, or because its wild growth furnished fuel, thatch and fertilisers to the neighbouring settlements ; or the applicants themselves had not adequate means to develop the land, and merely wished to lease it for speculative purposes. Though the Government declares its intention to honour as many applications as possible, still the impression remains that we have here a policy of expansion cramped by precedent established in a small crowded territory where land has more value than men ; that such a policy ought to be superseded by the true colonial outlook of a new territory, where men are more valuable than land, where land is useful primarily to attract men, and therefore should be given to them freely, coupled with additional inducements to settlers, like free transportation of family and effects. So much for the ten-year lease system in Korea.

The same short-sighted, uncolonial policy is revealed in the sale of land ; for the Government has gone into the real-estate business, which it conducts upon canny principles. It operates through the Oriental Development Company, a big stock company organised in 1908 by the Japanese Government and private shareholders to exploit the resources of Korea. The Government nominates the President and two Vice-Presidents, and it holds nearly one-third of the capital stock, for which it has put in an equivalent in State-owned lands. It also pledges itself to lend the Company financial aid not to exceed £30,000 sterling annually for the first eight years. The "Rule for Settlers," issued by the Company in September of

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1910, is significant, because it embodies the old dwarf land scale current in Japan. Settlers may become either peasant proprietors or tenants. The first class may lease paddy fields and dry fields to the total amount of 5 acres per family, and by the payment of annual instalments become owners in twenty-five years. These instalments are fixed high enough to cover in this period the current market price of the land, which averages about £5 sterling per acre for dry fields and from £8 to £10 sterling for paddy, and in addition a 6 per cent. interest on its value, which is considered as an advance. Meanwhile the settler must pay the land tax and all other dues upon his leasehold. Tenant settlers pay a fixed rent for their land, but they are allowed to buy it eventually, if they prove to be respectable and industrious. Peasant proprietors who have given proof of a similar character may apply to the Company for the purchase of land up to 12½ acres, including their original holding ; but this estate represents the rather pitiful summit of colonial ambition in Korea.

Nor are these the only restrictions. Settlers must be over twenty years of age, free from military service, and must bring their families with them. The Company in turn will advance them money up to £20 per household for their initial expenses, such as building the home, but it exacts 7 per cent. interest on the loan and repayment by annual instalment in twenty-five years or less.

What was the response to these munificent colonial offers on the part of Japan, which has to-day about 420,000 enterprising subjects living in foreign lands ? In the year 1910—11 the Oriental Development Company received 1,235 applications from would-be settlers ; only 160 of these were approved, and they received barely 4 acres per family. The Thirteen Colonies and Canada and German Poland would have made slow progress by such methods. The official explanation of this apparent reluctance to carry out the avowed purpose of the Company is another and higher purpose to admit only model farmers from Japan, in order to set up a high standard to Korean cultivators. Various applicants were therefore rejected because of their inexperience in agriculture, or inadequate capital, or undesirable personal character. To the outsider, the big national land reserve here, as in Sakhalin, is being handled as if it were the asset of a business corporation looking to well-secured dividends. The larger national enterprise of genuine colonisation,

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with its rapid increase of national wealth, is neglected, in this petty retail trade.

But there is the other consideration of the native Korean population and the improvement of their tillage methods. The Japanese settlers are to be glorious examples. This is not a congenial *role* to colonists. Indeed, the efficient colonist is often an untamed spirit, impatient of restraint, rebellious against precedents, but keen for making his own methods on a remote or dangerous frontier like the unpeopled Diamond Mountains of Korea or the savage border of the Formosan Highlands. Moreover, the glorious examples will prove effective means to stimulate backward Korea only when they are numerous enough to intensify competition. Competition is brutal, but it is the only method that can be relied upon to do the work. Moreover, there is another question—whether Japan can afford the slow and costly process of raising those thirteen million Koreans to the Japanese standard of efficiency, till they can finally reinforce her strength, or whether it were wiser to transplant Japanese colonists on a big scale to the unoccupied public lands of Korea, there to increase to a second Japanese nation. There given a free hand, encouraged in their spirit of initiative and not thwarted, they can be trusted to develop a colonial type of character with the comprehensive mind of colonials—men of vision for big national schemes, of power to instil into the nation new ideals and new forces of political expansion, such as Japan will need in the future, if she is to hold her own in the East.

From this abortive system of colonisation we turn with admiration to Japan's administration of Korean affairs and her efforts in behalf of her Korean subjects. We see her sending the whole nation to school and putting it through a regular curriculum with a view to graduation. Her task is easier than similar national tasks in the Philippines, Java and India, because Japan is dealing with a kindred people, living in a neighbouring territory under familiar climatic conditions, having an allied civilisation, and, what is more important, capable of being embodied in the body politic of Japan. The question which the Japanese authorities are facing with intense earnestness is this : Whether the despised Koreans, a " Nation of Cowards," as they have been called, brought to the verge of racial and political decay by centuries of misrule, are susceptible of such moral, physical,

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intellectual and industrial regeneration as will make them a force in Japan's struggle for military and economic supremacy in the Far East. That they can be so regenerated is Japan's hope. The Koreans are regarded as a new national resource to be scientifically developed. Therefore Japan is attacking her problem with a courage, devotion and insight that argue well for her success. She is trying to correct the early mistakes of 1905, when camp-followers and riff-raff streamed into the country in the wake of the army of occupation, before military control had been superseded by an orderly civil government; and she is entrusting the difficult task of reorganisation to officials of high character.

The Koreans are docile, amiable, lazy, ineffectual. Centuries of oppression and over-taxation have robbed them of all economic incentive. Their ideal is "honourable idleness"; and to them all idleness is honourable. The coolie class is physically well developed and capable of severe labour; but these, like the middle and upper classes, seem possessed by a deadly inertia. A high official in Seoul said recently to the writer: "These people lack will power. All we can do is to teach them better methods of agriculture and industry, secure to them the profits of their labour, and thus gradually lift them. But the hardest thing will be to supplant their ideal of idleness by one of activity."

To this end the Japanese are devoting their attention to the children. They seem to have adopted for their motto Emerson's great saying: "Youth comes ever towards us with salvation in its hands." Common schools are being rapidly established over the whole country, middle and normal schools in the towns and cities, agricultural schools and experimental farms in various places, and agricultural classes embodied in academic courses of study. Textbooks compiled by the educational department are sold for a few pennies or distributed free. For the first time, apart from what the mission schools could offer, the Korean girls have an equal chance with the boys. Think of the boon to that unrecognised half of the Korean race, which was previously deemed unworthy to receive personal names! Their education consists in a wise combination of common school branches and varied industrial training; and it calls forth an eager response that is, to the onlooker, almost pathetic in these little daughters of a dulled, sodden, nerveless race.

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The boys' schools provide not only a full course of study, but every stimulus in the form of physical training to overcome Korean inertia. Military drill, football, baseball and tennis are pushed to the front, in order to arouse ambition and the corps spirit, which the native lacks. The purpose to develop in the Korean the manly qualities of the soldier is everywhere apparent. The writer asked an army officer in Pyong-yang Province: "Will you ever be able to make soldiers out of the Koreans?" He replied: "I don't know, but I hope so, if we can begin their training young enough; but up in this section, near the frontier, we must have wholly reliable troops."

The traveller in Korea is impressed by the ineffectuality of the people, their blank and purposeless expression, their feeble and awkward use of tool or implement, their inability to direct their muscular effort except in crude coolie labour. He listens, therefore, with surprise and some admiration to the Governor of Pyong-yang, an intelligent and big-hearted man, who maintains that the incapacity of the Koreans is the result of neglect, that with training they can attain the same technical efficiency as the Japanese, and under proper guidance their products can soon enter the world markets. And as, all eagerness and hopefulness, he described the methods by which he expected to accomplish this result among his people, one felt that this ruler of a remote northern province was an artist and an economist, but above all a teacher. Devotion, courage, and dogged perseverance in the application of tested methods of development characterise the Japanese officials in Korea. The quality of the natives would not lead an outsider to envy these national educators their task.

Education goes on also outside of school and workshop. The Japanese have accomplished wonders for the public hygiene. Medical schools, hospitals, vaccination stations and modern water-works for the big cities are being rapidly established. Sanitary associations attend to cleaning streets, removing garbage and sewage, and raising the standard of cleanliness on private premises. The reeking, filthy, malodorous Seoul which Mrs. Isabella Bird Bishop described in 1894 is a thing of the past. In the towns and the neighbouring country districts, a continuous spring-cleaning goes on under the supervision of the police, those able apostles

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of Japanese civilisation. Though only the streets and courtyards of the rural village come under their sanitary jurisdiction, they manage somehow to inject the spirit of cleanliness also into the interior of the mud-walled homes.

The well-water in Korean towns often causes epidemics, owing to infiltration from stagnant drains and cesspools. The water from the small polluted streams in the thickly populated coastal plains is little better. Therefore modern waterworks and filter systems have been built in the five large cities, either by the central Government or by the Japanese municipalities with Government aid. After the installation of the waterworks in the city of Pyong-yang, it became necessary to forbid by law the sale of the foul river water by the public carriers, in order to force the people to use the pure supply.

The law has had to step in also to educate the Koreans in a respect for forests. For centuries the hills and mountains, now bare and often eroded down to the underlying rock, have been stripped of their trees for fuel. Large timber has disappeared, except for a patch here and there. The young sprouts from the old stumps have been regularly cut in the fall and tied into bundles for firewood. Now all is changed under the direction of an expert Japanese forester. Though he has been at his task only since 1907, already little pine trees begin to dot the surface of the bare slopes. But these infant forests have to be policed, a difficult undertaking in a large area, for the Koreans still resort to their disastrous practice of cutting the saplings, and only by repeated arrests can they be taught to respect the integrity of the forests. Everything is done to encourage the natives to reforest their own waste lands. Young trees to the number of half a million and many bushels of seeds were freely distributed in 1909 from six forestry stations, and object lessons are furnished in the model afforestation carried out on terraces or slopes about Seoul and other large cities. The little pom-poms of green that now dot the bare hills and give promise of future forests are typical of Japanese efforts for improvement in this decadent country.

In Formosa, colonial administration found a complex problem, for which in the main it has worked out a wonderfully successful solution. When Li Hung Chang, the Chinese Plenipotentiary,

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handed over Formosa to the Japanese at the Shimonoseki conference in 1895, he offered them his condolences on their new acquisition. Under Manchu rule, Formosa had been notorious for the turbulent character of its Chinese population, the widespread brigandage, the prevalence of the opium habit and fatal malaria, and finally the head-hunting savage aborigines, who were a constant menace to economic development. The outlook was not encouraging.

The Japanese found the area of the island about equally divided between three million Chinese who had appropriated and tilled the fertile western lowlands, and the Malay savages, who, though numbering little over a hundred thousand, maintained themselves in the fastnesses of the eastern mountains, and from this base made depredations upon the frontier. The Chinese population contained many fugitive lawless elements, who either reformed amid the large opportunities and free life of this colonial island, or were recruited into the bands of the professional brigands. These bands were also reinforced at the time of the Japanese acquisition by insurgent Chinese who refused to submit to the new authorities. But the security to life and property guaranteed by the orderly Japanese rule appealed to the industrious Chinese and gained their allegiance for the Government. The brigands were either offered pardon and then systematically employed in some of the rough work incident to organising a new country ; or, where incorrigible, were brought under control by force. To-day the country is quite free from them, where only ten years ago they assaulted the capital, Taihoku, and necessitated an armed guard for every excursion beyond the city walls.

The opium evil has been curbed with equal success. To this end, the production and manufacture of opium were made a Government monopoly ; thus the quality, quantity and distribution of the drug was controlled. Then the efficient Formosan police took a census of those addicted to the habit ; thereafter only confirmed smokers were allowed to buy the drug, and that only in specified quantities, for the sale goes on under strict police surveillance. As a result of this method, the annual returns of the consumption of opium and of the number of smokers show a parallel decline, as the older victims of the habit die off. Thus

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the number, which was 170,000 in 1900, dropped to 130,000 in 1905.

The Japanese have coped with insurgents, brigands and opium smokers, but they have been in the main worsted by the swarms of anopheles mosquitoes breeding in the widespread paddy fields of the Formosan plain. Malaria is responsible for about 20 per cent. of all the cases of sickness in the island, and for probably 10 per cent. of the mortality. In the capital, Taihoku, it is being successfully combated by a new model sewerage system and water-works, so that this city has become comparatively a safe place of residence. Other towns are following its example. For this reason, the Japanese immigrants, who are found largely in the towns and cities, show a smaller mortality from malaria than do the Chinese, who are distributed through the irrigated plain. Malaria may explain the constant exodus of Japanese from Formosa and the small number of immigrants who remain in the island; their marked appetency for the liberal professions, trade, and technical industries,—all occupations appropriate to urban life; and their apparent reluctance to engage in agriculture, stock-raising, forestry, and even fisheries, all which together employ only 1·4 per cent. of the Japanese population of Formosa. It may be that malaria is going to prove the great obstacle here to genuine colonisation, since it is chiefly the Japanese farmer class that needs an outlet.

The expansion of Japanese control into the Malay mountain section of Formosa, and the consequent problem of a belligerent savage frontier, are the result of interesting economic and geographic factors. That expansion has been stimulated and directed into the mountains by the growing demand in the world markets for camphor and tea. Ordinarily savage tribes are long left in undisputed possession of their mountain lands, because these usually repay cultivation only under heavy pressure of population from the plains. But the mountains of Formosa, on the contrary, have attracted expansion, because they commanded a practical monopoly of the camphor industry, which has a limited activity elsewhere only in Japan. The *Raubwirtschaft* methods of the Chinese have, since 1790, steadily destroyed the camphor fields without replanting. Beginning at that time on the eastern frontier of the agricultural lands in the plains, they have pushed their camphor camps first into

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the foot-hills and then into the mountains, cutting down the forests and driving the savages farther into the highlands. The camphor question and the savage question are, therefore, identical, for the hill tribes, who resent this steady encroachment, raid the camps, destroy the stills, and massacre the workers at every chance, incidentally carrying off the heads of their victims, according to respected ancestral usage, to grace a wedding feast or a funeral ceremony.

As the forests fall and the camphor workers advance, in their wake come the tea planters to occupy the cleared lands. Hilly ground with an altitude ranging between 1,000 and 4,000 feet, a hot climate with an annual rainfall of 80 inches or more, and a light sandy loam soil combine to form ideal conditions for tea culture. These conditions were found in or bordering upon the savage territory of Formosa as early as 1868, and since that date they have been steadily exploited, at first under an official or volunteer guard of armed Chinese, and recently under the protection of the more efficient Guard Line of the Japanese ; because the frontier of settlement is always the lurking place of the head-hunting savages, ready to commit depredations upon the frontier villages.

The needs of the camphor industry, which yields a large revenue as a Government monopoly, and the expanding tea culture, which, in 1908, accounted for over two-thirds of the total export receipts of the island, together keep the Japanese Guard Line constantly on the advance into the savage wilderness. In 1904 some 300 square miles of territory were reclaimed for civilisation and development.

The Guard Line is a border outpost defence, designed to protect the settlements and curb the aborigines. Its striking and original feature is a wire fence, 5 feet high, the lowest wire of which is charged with an electric current strong enough to stun or kill anyone trying to climb over or creep under it. It stretches for over 300 miles through the mountains along the savage border, and it is guarded about every 500 yards by block-houses, in which are stationed armed police. The fence is cleared of brush for 30 feet on either side, so that no one can approach without detection. The savages are warned of the danger in it, and consequently few of them have been killed ; but the fence serves to confine them in their own area, and to cut them off from outside supplies, especially from salt. Thus

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the Japanese are able to put on the screw. Under the pressure of this need, tribe after tribe has submitted to authority, given up their arms, and allowed their territory to be included within the Guard Line. In return, they are given agricultural implements, seed, land for cultivation, and medicines when they are sick. They relinquish head-hunting, but in deference to tribal ideals use monkey skulls instead. The right of trade is conferred as a privilege upon certain tribes or certain individuals; it is restricted to necessary articles, from which firearms and ammunition are specifically excluded. It is cut off immediately if the rules are violated, or if the tribe become unruly. The agents in this wise and humane conquest of the savages are the police in the block-houses. These carefully chosen and carefully trained officers of the law distribute medicine to the sick, supervise disinfection, teach the savage children the rudiments of the Japanese language and manners, control barter between the savages and the common people, prevent the sale of guns, take observations upon aboriginal life, and gather information about tribal affairs. They exemplify the genius of the Japanese for adapting means to ends.

Economy of administration and the intelligent development of local production and industries enabled Formosa to be self-sustaining as early as 1908, so that the small subsidy annually granted by the hard-pressed national treasury in Japan could be dispensed with two years before the allotted time. The long list of Government monopolies in Formosa (opium, camphor, salt and tobacco) and of Government enterprises, such as railroads, camphor refineries, and irrigation works with hydro-electric power, have helped to achieve this end. But these monopolies naturally raise a question as to the effect upon opportunities open to Japanese immigrants.

The riff-raff from the home islands that flocked to Formosa, as to Korea, after the acquisition made difficulties for the Government and led them to discourage further influx. Those who came found the level land suitable for agriculture already fully possessed by the anopheles mosquito and by the industrious Chinese, who showed a population of 415 to the square mile in the civilised area. The most lucrative occupations were already in the hands of the Government and of the Chinese. The door of opportunity was closed in the new colonial land. This was the condition which the would-be

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colonist found upon arrival ; so he went back home. Hence the striking fact about Japanese immigration into Formosa is the excessive emigration, which steadily increased from 1898 to 1904, as the following table shows :—

Year.	Arrivals.	Departures.	Increase.
1898	13,214	3,078	10,136
1899	20,743	7,903	12,840
1900	20,995	8,842	9,704
1901	17,841	14,054	3,787
1902	13,821	11,478	2,343
1903	15,892	13,149	2,743
1904	11,564	12,158	—591

The excess of departures over arrivals in 1904 is doubtless due, in part, to the recall of reservists for the Russo-Japanese war ; but the table as a whole points to some persistent cause of discouragement to immigration. So do other statistics. The number of Japanese who, prior to 1905, had come more than once was only 2,383, or 2 per cent. of the total number of arrivals given in the table. This means that only a scant proportion of the big number who left ever came back. Apparently their survey of conditions in the Island or their taste of Formosan life was not reassuring. It is furthermore significant that while women, from 1898 to 1905, constituted about one-third of the annual arrivals, in 1901 and after they formed over one-half of the annual increase. This is contrary to normal colonizing experience, which shows always a marked predominance of men.

At the end of 1905, after ten years of Japanese occupation, the Japanese citizens in Formosa numbered only 57,309, or less than 2 per cent. of the total population. Government offices and the liberal professions employed 40 per cent. of these, while agriculture, forestry and fisheries altogether engaged only 1·4 per cent. This is the striking anomaly in Formosa, that a people with a gift and love for farming should avoid the occupation native to them and natural to every normal colonist. The census of the population in Formosa for 1905 states that “ this scarcity of agricultural immigrants, in spite of rich agricultural resources, is chiefly due to general ignorance of the conditions of the Island ” ; but this reason

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is not convincing. In a country where almost every person can read, and where newspapers are abundant, this ignorance could not long endure. Moreover, the 70,569 Japanese who went to Formosa and left again between 1898 and 1905 are certainly exempt from the charge of ignorance.

Making due allowance therefore for the competition of the Chinese and the effects of malaria in a country of paddy fields, swarming with mosquitoes, but also taking into consideration the extensive area of more healthful mountain land in Formosa reclaimed from the savages and devoted to tea culture, in which the Japanese excel, one cannot escape the conclusion that here, as in Sakhalin and Korea, Japanese colonial methods fall short at a vital point.

MODERN MATERIALISM

By W. McDougall, M.B., F.R.S.

MR. H. S. ELLIOT having demolished, to his own satisfaction, the "Illusions of Professor Bergson," has turned to give the final blow to "Modern Vitalism" by criticising in the October number of this Review my book *Body and Mind*. For he describes that book as "the most efficient defence of animism (and by implication of vitalism) that has ever been published"; and he holds, therefore, that if the arguments of that book "can be shown to have no weight the case for Vitalism will be lost for ever." Although I do not believe that the fate of Vitalism will be determined by the success or failure of Mr. Elliot's spirited attack, and would not be taken to accept his flattering estimate of the importance of my book, I am glad of the opportunity of making a brief reply and of adding a few words on present-day Materialism as expounded by other writers.

Let me first say in all seriousness that in my opinion Mr. Elliot deserves our gratitude for his frank affirmation of Materialism and of the mechanistic dogma. For his publication may help in dispelling a delusion widely prevalent at the present time; the delusion, namely, that science has now definitely emerged from and outgrown the materialistic phase which it had assumed in the second half of the nineteenth century. If this opinion is fallacious; if it seriously over-estimates the degree to which the attitude of science has changed in recent years; if the appearance of harmony between science on the one hand and religion and philosophy on the other is largely deceptive and due more to reticence on the part of scientific men than to any real *rapprochement*; then, I think, it is well that the delusion should be dispelled and that we should realise more accurately how we stand.

A chief part in the production of this appearance of reconciliation

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has been played by the doctrine of psycho-physical parallelism in its various forms ; for the acceptance of this doctrine enables the materialist to claim a place under the respectable banner of Idealism, and the idealist to accept the conclusions of Materialism. My own book was in the main an attempt to show that all forms of the parallelistic hypothesis are untenable, that the reconciliation of mechanistic science with spiritualistic or idealistic philosophy which it claims to effect is illusory, that belief in the universal sway of mechanical laws is incompatible with the belief in the efficacy of intelligent purpose, and that, therefore, we still stand before the old dilemma Materialism or Spiritualism.

I regard, therefore, as timely and welcome, not only Mr. Elliot's publications, but also the, if possible, still more frank and dogmatic affirmation of Materialism by Sir Ray Lankester,* and the recent pronouncements to the same effect of Professors Schaefer † and Loeb. ‡ For all these serve to show us where we stand, and, by rudely dispelling the clouds which have obscured the issue, must spur on our thinkers to face the old problem anew, and either to choose deliberately between the horns of the dilemma, or to discover some new way of reconciliation less unsatisfactory than any of the hypotheses that are commonly and conveniently grouped together under the head " psycho-physical parallelism."

How, then, does Mr. Elliot show that my arguments in favour of " Vitalism " have no weight ? A large part of my book consists in an exposition of the inadequacy of mechanical conceptions to explain the course of vital and mental processes. All this long and cumulative argument he disposes of in the following way. The vitalists, he says, postulate a " vital force " § to explain whatever phenomena of organisms present difficulties to the mechanistic system of explanation. And he loudly complains that, when the materialist demands evidence of the non-mechanical factor, the

* Introduction to Mr. Elliot's *Illusions of Professor Bergson*.

† " Life : Its Nature, Origin and Maintenance." Presidential address to the British Association, 1912.

‡ *The Mechanistic Conception of Life*. London, 1912.

§ I have not used this expression—but I am willing to adopt it on the understanding that it serves merely to indicate a problem, to mark the need for the recognition of some factor or factors, or mode of process, other than those recognised by physics and chemistry.

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vitalists merely exhibit the difficulties in the way of mechanical explanation. But what would he have? Does he demand that the non-mechanical factor or factors—call it for convenience “vital force”—shall be exhibited in a bottle of spirit, or in a series of microscopic sections, or otherwise presented to his senses? Does he not know that many—in strictness, all—the things and forces or energies by whose aid the physicist explains the flux of phenomena are imperceptible; that they are not perceived, but only conceived; that these conceptions have been achieved by an effort of the imagination; and that the only justification for any conception of this kind is that it fills a gap in the system of explanation better than any other conception? That, in short, the value or utility or validity of any one of them is measured by the inadequacy of those others to furnish a complete system?

Still, Mr. Elliot's complaint would not be altogether pointless if, as he implies, vitalists did no more than indicate certain facts or processes which have not yet been mechanically explained. But they have not been content to point to such facts. Nor are they content to remind us that no single physiological process, neither muscular contraction, nor nervous conduction, nor assimilation, nor secretion, nor excretion, nor respiration, nor growth, nor reproduction, nor any other, has yet been adequately explained in terms of physics and chemistry. They insist rather upon certain processes to which the mechanical explanation is inadequate, not only in fact, but also in principle. In my book I attached great importance to a process of this kind which has been abundantly demonstrated by Professor Hans Driesch (amongst others), who has also insisted strongly on its significance.* The facts, and the conclusion to be drawn from them, may be briefly stated as follows. The mechanist regards the development of the egg to the form characteristic of the species as determined at every step by the physico-chemical structure of the egg; *i.e.*, by the constitution or nature of the material particles which make up the substance of the egg and by the spatial distribution and relations of these particles within the egg. Yet the spatial distribution of these constituent parts of the egg, or of the embryo at various stages of development, (and therefore their reciprocal

* “The Science and Philosophy of the Organism.” Gifford Lectures, 1907—8.

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influences upon one another) may be (and in some experimental instances have been) profoundly altered, without preventing the development of the egg to a complex multicellular organism having all the characteristics of the species. The mechanist who gives up Materialism and regards matter, extension, and all spatial relations, as phenomenal only may, by so doing, weaken or soften the impact of this argument upon his position; though at the same time he greatly weakens, or resigns altogether, most of the other grounds for the rejection of psycho-physical interaction (as we see in the case of Professor Ostwald).^{*} But Mr. Elliot is not a mechanist of this type; he is an uncompromising materialist; for him "all matter is reducible to atoms, and all energy to matter in motion," "we may look upon an atom in motion as the unit of all physical phenomena."[†] It is therefore incumbent on him, and on all who think with him, to take note of this argument, and to show, if possible, that it does not, as it seems to do, prove the mechanistic explanation of ontogenesis to be in principle impossible. But Mr. Elliot (like the others)[‡] persists in ignoring this argument as completely as the many other arguments of similar tendency which he might have found in the pages of my book. I would therefore beseech him to ponder this one argument and to give the world the benefit of his reflection upon it.[§]

Mr. Elliot's complaint that no one has rendered "vital force" perceptible to his senses is not his only ground for rejecting "vital force"; he rules it out by way of an argument, the premises of which are assumptions that beg the question in dispute. Mixed inextricably with this argument is the assertion that the material-

^{*} *Vorlesungen über Natur-Philosophie.* Leipsic, 1902.

[†] "Modern Vitalism." BEDROCK, Vol. I., 3, p. 315.

[‡] One defender of the mechanistic hypothesis has courageously attempted to meet this argument, namely, Dr. J. W. Jenkinson ("Vitalism," *Hibbert Journal*, Vol. IX.). But the failure of the attempt must be very obvious to every unprejudiced reader, and has been sufficiently demonstrated by Mr. L. Doncaster ("Vitalism," *Science Progress*, Vol. I.).

[§] Since the printing of this paper I have seen Mr. Elliot's article, "The Spectre of Vitalism," in the January number of *Science Progress*. In that article Mr. Elliot has made some attempt to meet this argument; but his remarks show that he has failed to grasp the point of it, and I must therefore persist in inviting him and those who think with him to consider it more carefully.

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istic position is justified by the law of the conservation of energy ; for Mr. Elliot puts aside the opinion expressed by a number of eminent physicists to the effect that this law does not rule out mind or spirit from all participation in the course of events ; he " knows " better. But the argument when reduced to order will be found to consist in the following propositions.

Every form of influence that operates in the world is properly called force. All force is the motion of particles of matter. Therefore the course of all events is wholly determined by the reciprocal influences of particles of matter, and there can be no other types of process, activity, or influence in the universe. If we ask what guarantees Mr. Elliot can offer for the truth of the premises of his very conclusive and correct piece of reasoning, we shall, I presume, find that he simply " knows " them to be true, just as those who affirm other dogmas " know " them to be true ; that is to say, " suggestion " has established them in his mind so stably that he holds them with a conviction which is proof against all reasoning and renders it impossible for him to adopt even for one moment a critical attitude towards them.

There follow some pages inveighing against my remarks on teleology. I had insisted that striving towards an end seems *primâ facie* at least as fundamental and as intelligible a type of process as being pushed from behind ; I had especially protested against the fashion of reconciling the reality of purposive activity with the universal sway of mechanical causation by asserting that they are identical processes, a fashion which seems to me merely to obscure a fundamental difficulty ; and I had pointed out that very many biologists at the present day are of the opinion that the theory of natural selection does not suffice to explain away all the seeming evidences of purposive activity in the world. Here Mr. Elliot assumes towards me an attitude of kindly commiseration ; for does he not " know " that natural selection is all-sufficient ! To my contention that natural selection presupposes the struggle for existence and, therefore, the power of striving on the part of organisms, he replies by pointing to characters (the shape and colour of a butterfly and the odour of the carrion-flower) which are of the kind most readily accounted for by the theory of natural selection. I reply that, looking like a dead leaf, or smelling like carrion, is not an

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exhaustive enumeration of the activities of the organisms in question. Further, if we admit the plausibility of the theory as applied to explain the acquisition of these characters by the species, we should still like to know how the tiny speck of protoplasm which is the butterfly's egg manages to assume the appearance of a dead leaf. Mr. Elliot's reply to this demand has been given in his article. "Why be ashamed," he says to the vitalist, "of saying that we do not know why it develops?" This is unkind and a little unfair. It is Mr. Elliot and his like who claim to know; for they tell us that the nature and spatial arrangement of the atoms of the egg are such as to determine just this development; whereas I have not merely confessed without shame, but have even shamelessly asserted, that we do not know how the thing is done.

To my contention that to liken an organism to a machine does not deprive it of its purposive character, because all machines are embodiments of human purpose, Mr. Elliot replies by saying that innumerable machines, "identical in essential structure and function," with steam engines and motor-cars occur naturally, that, *e.g.*, the earth is "a machine for catching meteorites," and the moon a machine for raising tides on the earth. This is the merest quibbling. When the mechanists seek to give plausibility to their view of the nature of organisms by likening them to machines, it is, as Mr. Elliot very well knows, machines of human manufacture, gramophones, steam engines, printing presses, clocks, and so forth, to which they refer; and to assert that the moon is "identical in essential structure and function" with a motor-car is mere prevarication.

Up to this point in Mr. Elliot's article I find nothing to complain of except the peculiarly extensive character of his "knowledge"; but there follows a passage against which I beg leave to protest. In a footnote, which has no necessary connection with my arguments, I went out of the way to show that those are wrong who maintain that it is impossible to imagine events which would constitute convincing evidence that human personality may survive the death of the body. I did this by constructing an imaginary series of incidents which, if they were to occur, would, it seemed to me, constitute such evidence. Mr. Elliot cites for the amusement of his readers the most extravagant incidents of my imaginary case

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“as an instance of the extremity to which vitalists are reduced in their attempt to find arguments against mechanism”; and he adds “I am not going to comment on this passage.” I would suggest that this is hardly “cricket.” It might, or might not, pass as fair in the law courts, but it is hardly worthy of the pages of a scientific review. He has totally ignored by far the greater part of my argument, and yet has found space to insinuate that I adduce as evidence a series of imaginary events which was avowedly put forward as an extravagant fancy merely.

The concluding pages call for only one comment from me. If Mr. Elliot chooses to assert that the most modern conception of the soul is that of “a thin vapour of smoky consistency, more or less spherical in shape” (p. 331); and that belief in the existence of God is a degrading type of Materialism (p. 332), that is a matter for the editors rather than for me. The comment I wish to make is that, after frankly adopting Epiphenomenalism in his book on M. Bergson, Mr. Elliot now skips lightly over to Psychical Monism; that is to say, since Mr. Elliot wrote his book, he has learnt the trick beloved of modern materialists. Convinced of the primacy and sole efficacy of matter, it hardly seems to them worth while to follow the metaphysician in observing the distinctions between the various psycho-physical hypotheses which agree in denying psycho-physical interaction; but they find it convenient for controversial purposes to shift their ground at will from one to another, and, since the rules of verbal logic permit of it, to assert, with just a suspicion of a tongue in the cheek, that they also are idealists. But in his book on M. Bergson,* Mr. Elliot, after honouring me by an appreciative reference to my *Body and Mind*, proceeds to brush aside all my reasoning in favour of psycho-physical interaction by pointing out that I have advanced only a few arguments against Epiphenomenalism (the form of Parallelism accepted by him in that work), and that none of them is conclusive. Now, in writing *Body and Mind*, I was content to concentrate my effort on the criticism of the most subtle, respectable, and influentially supported form of Parallelism, namely Psychical Monism, and to pass over Epiphenomenalism very briefly; partly because Professor Ward had, as it seemed to me, finally

* Pp. 187 *et seq.*

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refuted it,* and I regarded it as no longer a living issue ; but also because, as I pointed out, most of my arguments made against Epiphenomenalism even more conclusively than against any other form of Parallelism.† In fairness to my work, Mr. Elliot should have noticed this, instead of mentioning merely my few remarks directed specifically to Epiphenomenalism and adding “ that is all that Dr. McDougall has to say against the theory.” And, now that Mr. Elliot has embraced Psychical Monism, it should be clear to him that he cannot hope to show that my contentions “ have no weight ” and that therefore the case for Vitalism is lost for ever, unless he make some attempt to meet the elaborate and cumulative argument of my book against Psychical Monism. Many of the most important parts of this argument he has not even mentioned ; ‡ and by ignoring them and confining himself to harping on the wearisome old refrain “ vital force explains nothing,” he has seriously misrepresented my work.

But enough of Mr. Elliot. Let me seize the opportunity to say a few words about the reasoning of some of the acknowledged leaders of biological Materialism.

Professor J. Loeb is the acknowledged prince of mechanists among the biologists. He has made many discoveries of great interest and importance ; but I venture to think that he and many others have drawn from his observations conclusions which are not in the least warranted. Whoever in these days advances arguments against the mechanistic view of life is apt to find himself countered with the remark, “ Ah yes, but Loeb has shown that the eggs of sea-urchins can be made to develop without fertilisation by the addition merely of certain inorganic salts to the water by which they are bathed ; and has thus proved that the development of every egg is purely a physico-chemical process.” Professor Schaefer in his recent address cites these observations, saying that Loeb has shown that it is possible “ to bring about the development of the whole body (of the sea-

* “ Naturalism and Agnosticism.” Gifford Lectures, 1906—8.

† This has been admitted by Professor Heymans, of Groningen, in a recent paper, in which he has defended Psychical Monism against my criticisms. “ In Sachen des psychischen Monismus,” *Zeitschr. f. Psychologie*, Bd. 63. Leipzig, 1912.

‡ E.g., the arguments from the unity of consciousness, from the distribution of consciousness, from “ meaning,” and from “ memory.”

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urchin)," by substituting a simple chemical reagent for the male element in the process of fertilisation. He immediately adds, "*Kurz und gut*, as the Germans say, vitalism as a working hypothesis has not only had its foundations undermined, but most of the superstructure has toppled over, and *if any difficulties of explanation still persist*, we are justified in assuming that the cause is to be found in our imperfect knowledge of the constitution and working of living material."* It will be noticed that it is Professor Loeb who gets all the credit; he is said to bring about the development of the ovum, whose share in the process is conveniently ignored. This is typical of the reasoning of many mechanists in relation to this and various other facts of observation. They build upon the experimental observation a mighty superstructure of assumption, which would hardly be justified if Professor Loeb had himself constructed the ovum out of simple inorganic salts. Regarded impartially, this much celebrated experiment adds nothing to our knowledge beyond the fact observed. It has long been known that many eggs may develop without the aid of the male element; and a strictly comparable fact has been familiar for centuries, the fact namely that the development of dry seeds of plants may be initiated by adding to them a little warmth and moisture. Why do the mechanists regard Professor Loeb's treatment of the ova as so much more significant than the success of the child who waters his little garden and so "brings about the development of the whole body" of his plants? Is it not owing to their inveterate habit of making inductions by simple enumeration? All the sea-urchin's eggs they had observed, up to the date of Professor Loeb's experiment, had developed only after fertilisation by a male element. Hence they had implicitly adopted as part of their stock of "knowledge" the proposition that the eggs of sea-urchins can develop only after fertilisation; and then, when one was observed to develop without it, they were "struck all of a heap," and, in the consequent confusion of their thoughts, they attributed to Professor Loeb the credit that was really due to the egg.†

* Presidential Address, p. 12. Italics are mine.

† Curiously enough, it is with the eggs of sea-urchins that some of the most striking experiments (of Driesch and others), demonstrating development to the specific form in spite of mechanical deformation of the egg or embryo, have been carried out. Why, then, do the mechanists, while continually

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Among many examples of such reasoning (or lack of reasoning) another may be cited from Professor Schaefer's address. He cites the movements of *Amœba* as typical of one great class of physiological phenomena, those of motion and locomotion, in animals. "We regard such movements as indicative of the possession of 'life'; nothing seems more justifiable than such an inference. But physicists show us movements of a *precisely similar character* in substances which no one by any stretch of the imagination can regard as living; movements of oil drops, of organic and inorganic mixtures, even of mercury globules, which are *indistinguishable in their character* from those of the living organisms. . . . It is therefore certain that such movements are not specifically 'vital,' that their presence does not necessarily denote 'life.' "* He adds that we cannot doubt that the actions of complex organisms have been evolved from movements of the amœboid type, "movements which can themselves, as we have seen, be perfectly imitated by non-living material." Then follows immediately the following passage—"The chain of evidence regarding this particular manifestation of life—movement—is complete. Whether exhibited as the amœboid movement of the proteus animalcule or of the white corpuscle of our blood; as the ciliary motion of the infusorian or of the ciliated cell; as the contraction of a muscle under the governance of the will, or as the throbbing of the human heart responsive to every emotion of the mind, we cannot but conclude that it is alike subject to and produced in conformity with the general laws of matter, by agencies resembling those which cause movements in lifeless material."

This astonishing piece of rhetoric calls for two comments. First,

harping upon Professor Loeb's experiment on the eggs of sea-urchins, as continually ignore these other experiments on the same material? It would, I think, be unjust to their moral qualities to regard this difference of attitude as due to wilful selection among the facts. The answer lies rather deeper; Professor Loeb's experiment, without actually lending support to the mechanistic position, has to a superficial glance the appearance of doing so. It has a "suggestive" force, which easily prevails over minds unaccustomed to reach conclusions by reasoning. On the other hand, the vitalistic conclusion indicated by the experiments of the other class can only be reached by a piece of genuine reasoning, a process to which few of the mechanists are accustomed or willing to trust themselves.

* *Op. cit.*, p. 9. Italics mine.

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the conclusion professedly drawn from the alleged fact, from the asserted identity of the movements of *Amœba* with those of certain inorganic globules, is by no means justified by the alleged fact ; the leap is an enormous one. One is tempted to regret that some of the conceptual chasms across which our agile mechanists so lightly leap cannot be given a physical form ; for, if that were possible, truth might be evolved by the direct operation of natural selection. Secondly, the alleged fact which is made the foundation of this enormous structure of assumption is one which has been very seriously blown upon. Mr. H. Jennings,* who is widely recognised as one of the most careful and experienced observers of the life of the animalcules, claims to have shown by direct observation that the movements of *Amœba* are radically different from those of any inorganic or non-living globules hitherto described ; and incapable of being fully explained by any physical principles hitherto suggested in this connection ; and he has made it seem highly probable (in fact, if we were to follow the example of our opponents, we might say "we cannot for a moment doubt") that those who have described the two kinds of movement as essentially similar were misled, by their mechanistic prejudice and by their desire to justify it, into basing a hasty conclusion on hasty and imperfect observations. There may possibly be room for doubt as to the correctness of Mr. Jennings' observations. But surely it was incumbent on Professor Schaefer at least to mention Mr. Jennings' work and to suggest this possibility ; especially in view of the fact that he addressed a predominantly lay audience.

Other great physiological problems are disposed of by Professor Schaefer in the same airy fashion : the problems of metabolism, for example, as follows. After mentioning that colloidal solutions contained in films of uncertain constitution occur in the body, and that, under similar physico-chemical arrangements made in the laboratory, interchange of substances takes place through the film, he writes :—

"It is true that we are not yet familiar with all the intermediate stages of transformation of the materials which are taken in by a living body into the materials which are given out from

* *The Behaviour of the Lower Organisms.*

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it. But since the initial processes and the final results are the same as they would be on the assumption that the changes are brought about in conformity with the known laws of chemistry and physics, we may fairly conclude that all the changes in living substance are brought about by ordinary chemical and physical forces."*

Another immense leap, which, like the others, takes off from an insecure basis. For the identity of the cell with the colloidal globules prepared by physicists is by no means established.

The most profound problem that confronts the biologist, one before which even some mechanists bow the head and acknowledge some measure of uncertainty, is disposed of by Professor Schaefer in the following passage :—

" Lest he (i.e., man) be elated with his psychical achievements, let him remember that they are but the results of the acquisition by a few cells in a remote ancestor of a slightly greater tendency to react to an external stimulus, so that these cells were brought into closer touch with the outer world ; while on the other hand, by extending beyond the circumscribed area to which their neighbours remain restricted, they gradually acquired a dominating influence over the rest. These dominating cells became nerve-cells ; and now not only furnish the means for transmission of impressions from one part of the organism to another, but in the progress of time have become the seat of perception and conscious sensations, of the formation and association of ideas, of memory, of volition, and all the manifestations of mind."

How easy ! How simple ! But, if we can steel ourselves against the subtle atmosphere of omniscience which rightly envelops those who listen to the address of a President of the British Association, how superficial !

These are fair samples not only of the reasoning of " the Presidential Address," but of many other publications of the mechanists. Whenever they are able to point to any new evidence that any particular chemical substance, or a physical process of any particular type, is involved in physiological process as a seemingly indispensable factor, they triumphantly point to the fact as yet another complete refutation of Vitalism. But their attitude would be justifiable only over against some unknown species of vitalist who should maintain that the chemical properties of living

* *Op. cit.*, p. 11.

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matter are indifferent, and the laws of physics and chemistry completely set aside, within the boundary formed by the skin of a living animal. Everywhere, in place of demonstration or careful reasoning, we are carried over immense gaps in our knowledge by such phrases as "nor can we for a moment doubt," or "we cannot but conclude," or "we may fairly conclude," or "it will without doubt be found," or "we are compelled to believe." But, when any reader is so unsympathetic as to enquire why "we are compelled to believe" the far reaching propositions in question, he may look in vain for any train of reasoning from established premises which even makes any show of carrying him to these conclusions; and he will be driven to adopt the explanation which Professor Schaefer offers for one of the current beliefs of this order, a belief which he himself, as it happens, has not acquired—"so much are we liable to be influenced by the impressions we receive in scientific childhood."*

Professor Loeb, the acknowledged high priest of the mechanistic creed,† has described, besides his celebrated experiment on the eggs of sea-urchins, many others which show him to be an exceptionally ingenious and successful experimentalist in the field of comparative physiology. But our just admiration for his experimental achievements should not blind us to the fact that, when he proceeds to draw far-reaching conclusions from his facts, his reasoning is generally contemptible. A single instance may suffice.

Professor Loeb has shown that some animals, exposed to a ray of light, turn either towards or away from the source of light; and he has applied to such behaviour the term "heliotropism," one long used by the botanists to denote the bending of plants towards the light. Hence, without more ado, he speaks of the "establishment of the identity of the reactions of animals and plants to light," and reasons as follows :—

"We have seen that, in the case of animals which possess nerves, the movements of orientation towards light are governed by exactly the same external conditions, and depend in the same

* *Op. cit.*, p. 19.

† Professor Schaefer himself has recently reviewed in the pages of *Nature* Professor Loeb's latest publication in terms expressing complete agreement with him.

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way upon the external form of the body, as in the case of plants which possess no nerves. These heliotropic phenomena consequently cannot depend upon *specific* qualities of the central nervous system."

Hence, he argues, "the irritable structures at the surface of the body, and the arrangement of the muscles, determine the character of the reflex act."* Again, of the complex instincts, we read :—

"Among the elements which compose these complicated instincts, the tropisms (heliotropism, chemotropism, geotropism, sterotropism) play an important part. These tropisms are identical for animals and plants."

Thus all reflexes and instincts are reduced to "tropisms," to simple direct chemical responses to physical or chemical stimuli. That is to say—having extended to certain reactions of animals the name tropism, which had been used to denote certain plant-reactions to which they bear a purely external and superficial resemblance, Professor Loeb holds himself justified in regarding reactions of these two classes as essentially similar or identical, although it is well known to him, as to everybody else, that they differ profoundly, if only in that a complex nervous system plays an essential part in the animal reactions, but is absent from the plants. Could our tendency to allow ourselves to be led into error by the mere sound of words be more wantonly indulged? And this is no mere slip of the pen. Professor Loeb has repeated this passage in substantially the same terms in several different publications. And it is only one particularly flagrant example of a mode of reasoning of which he is a frequent and prominent exponent.

So intoxicated is he with the adulation of his many admirers, and the success of the experiments by means of which he has shown that certain simple animals react in constant fashion to certain physical or chemical stimuli, that he goes far beyond most of the mechanists in their reduction of human and animal behaviour to mechanical sequences. Most of these are content to regard such behaviour as determined by complex conjunctions of sensori-motor reflexes, depending upon nervous systems so intricately constructed that hitherto we have elucidated with some approach

* *Op. cit.*, p. 68.

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to completeness only a few of the very simplest reactions.* Not so Professor Loeb. "I am concerned," he tells us, "to make the facts of Psychology accessible to analysis by physical chemistry."† In the concluding paragraph of the same essay he proposes to do the same for the science of ethics, by showing that the highest flights of moral effort are but "tropisms," direct reactions to chemical stimuli. And in various passages of several publications he implies the opinion (although, so far as I am aware, he has not explicitly asserted it) that the nervous system of animals should be regarded as a superfluous luxury, and even as a clumsy contrivance for effecting a number of reactions which, if Nature had but the cunning of some laboratory specialists, might with advantage have been secured without any such unnecessary complication of the mechanism.‡

Other mechanists also have of late been busy in the study of animal behaviour, from which they propose scornfully to reject all psychological and "metaphysical" notions as "unscientific,"

* Even this modest claim goes far beyond the truth; for the processes of nervous conduction, inhibition, facilitation, etc., are still completely obscure.

† *Die Bedeutung der Tropismen für die Psychologie*, p. 49. Leipsic, 1909.

‡ Lest readers not familiar with Professor Loeb's writings should suspect me of misrepresenting him, I cite the following passage from the lecture referred to above. After describing how certain insects turn and move towards the source of a ray of light directed upon them, he remarks: "The will of the animal, which in this case prescribes for it the direction of its movement, is the light, just as in the case of the falling of a stone or the movement of a planet it is the force of gravity." But he admits that there is a certain difference between the cases—"only the action of gravity on the path of the stone is direct, whereas that of the light on the movement of the insect is indirect, in so far as the animal is made to move in a given direction only by means of the hastening of chemical reactions." Other passages conveying the same implication may be found, especially in the essay on "The Physiology of the Central Nervous System" (*op. cit.*). At one point in this essay Professor Loeb seems to become momentarily aware that his facile methods are proving too much; for he asks: "Have we now to conclude that the nerves are superfluous and a waste?" He replies: "Certainly not. Their value lies in the fact that they are quicker and more sensitive conductors than undifferentiated protoplasm." But it is difficult to expose the method of Professor Loeb by citing passages, just because it consists essentially in a campaign of innuendo and suggestion against the functions of the nervous system, rather than in any attempt to reach conclusions by way of logical processes.

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regarding this as a step towards a similarly radical treatment of human conduct.*

Of all the controversies that have arisen in this field between the ultra-mechanists and those whom they stigmatise as psychologists and metaphysicians, the most interesting and instructive, perhaps, is that over the returning of bees to their hive. Dr. A. Bethe, a prominent mechanist, has undertaken to show that bees and ants are unconscious machines.† Now the homing of the bee from any spot within a radius of some miles from the hive is a task beyond the ingenuity of any unconscious machine hitherto imagined, if actuated only by the physical and chemical forces recognised by science. But to ascribe it to knowledge of the locality acquired by the bee would be to attribute to the bee a distinctly psychical power, the power of synthetically constructing from a multitude of successive sense-perceptions an ordered knowledge, some sort of mental plan or map, of the locality; a power which even the mechanists hesitate to attribute to any unconscious machine, and which gives the bees, if it is possessed by them, a high rank in the scale of psychical evolution. Now it has been demonstrated beyond the shadow of a doubt that many of the solitary wasps possess this power in a very high degree,‡ that they build up a detailed and comprehensive knowledge of an area some hundreds of yards in diameter, relying chiefly, if not solely, on visual perception. The female (of some species) prepares a series of nests in different spots for the lodgment of the series of eggs which she lays, and she provisions each nest in turn by bringing to it, sometimes on foot over

* Not that there is any doubt in their own minds on this last head. Hear Professor zur Strassen, one of the most prominent of them: "So until the opposite can be proved we must accept the proposition *that also human intelligence comprises no psychical factor*, and that it has arisen phylogenetically through continual transformation and refinement of physico-chemical nerve-processes" (*Die neuere Tierpsychologie*. Leipsic, 1908).

† "Dürfen wir den Bienen u. Ameisen psychische Qualitäten Zuschreiben," *Arch. f. d. ges. Physiol.*, Vol. 70, and "Heimkehr d. Bienen," *Zentralblatt f. Zoologie*, Bd. XXII.

‡ Superabundant evidence of this is adduced by Dr. and Mrs. Peckham in their delightful work, *Wasps, Social and Solitary*. It is true that M. Fabre has made a few experiments which, in his opinion, are incompatible with this view; but there can, I think, be no doubt that in this, as in other cases, this prince of observers has allowed his judgment to be disturbed by his desire to exhibit instinct as an insoluble mystery.

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very considerable distances, the insects or other small animals on which she preys ; and it is the minute observation of her behaviour in executing this task which has yielded a large part of the superabundant evidence referred to above. Other observers of the highest authority * have shown that the hive bees can not only find their way back to the hive, and that this depends upon visual recognition, but also that they can and do return again and again to those spots in which they happen to have found a supply of food. Yet in face of this overwhelming mass of evidence Herr Bethe deems it more truly scientific to postulate the existence of a form of radiant energy hitherto unrecognised by science than to credit the hive bees with any mental powers.† Therefore he assumes that this mysterious new force radiates from the hive and acting upon the bees (when and not until they are laden with honey or pollen) turns them (by way of a simple " tropism ") towards the hive, and so brings them safely home. Now, even if we put aside for the moment all the evidence (amounting to conclusive proof), that some at least of the Hymenoptera are really guided very largely by visual recognition, Bethe's hypothesis is not only so extravagant in itself, but, if consistently applied to all the facts of this order, involves so many further extravagant assumptions, that it crumbles under their weight. It must be assumed that the unknown force radiates not only from the hive, but also from any spot on which the hive has recently stood ; that a specific variety of this force is the peculiar property of each hive (for it brings bees not to any or every hive, but every one to its own hive) ; that it radiates also from any spot in which the bee has found a store of food and to which she returns ; and in the case of the solitary wasps, it must be assumed that it radiates from each nest in turn, just so long as it is not provisioned and sealed, and deserted for a new one ; also it must radiate from the prey which the wasp occasionally deposits in the course of her return to the nest while she makes an explora-

* Especially Lord Avebury, *Ants, Bees and Wasps* ; von Buttel Reepen, " Sind die Bienen Reflexmaschinen," *Zentralblatt f. Zoologie*, Bd. XX. ; Auguste Forel, *The Senses of Insects* ; M. Maeterlinck, *The Life of the Bee*.

† It should be added that Bethe reports a few experiments which seem adverse to the view that the bee's homing depends on visual recognition. But, when the peculiar nature of the vision of the compound eye is taken into account, these experiments appear quite inconclusive.

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tory excursion. In fact, if the hypothesis is to be workable, the unknown force must radiate from every spot to which the insect requires to return, and just so long as (but no longer than) this need obtains.

This instance of mechanistic reasoning is, I submit, peculiarly instructive ; because it shows to what lengths of extravagant assumption and to what degree of blindness to facts and the significance of facts some of the mechanists are led by their ill-founded prejudice. The biological materialist is commonly a laboratory specialist, and like Professor Schaefer,* he is apt to refer scornfully to those who do not share his prejudices as "biologists of the arm-chair and rostrum variety." May I, with all respect for the labours of the laboratory, suggest that a too exclusive devotion to them may actually be prejudicial to the acquisition of a philosophic or truly scientific attitude towards the great problems of biology ; that the art of reasoning requires cultivation no less than the arts of laboratory manipulation ; that the biologist cannot profitably excuse himself from the labours of the arm-chair ; and that he may with advantage desert occasionally both arm-chair and laboratory in order to make an excursion to the nursery, where he may hear propounded by the fresh voice of childhood some of the old riddles which the mechanistic scheme leaves as insoluble as ever. "Where does space come to an end ?" "When did time begin ?" "What was there before the world began ?" "Why can't I stop thinking ?" For these artless questionings may perhaps bring home to him the fact that the mechanistic scheme of things, imposing as it is, offers no self-contained and complete solution of the riddle of the universe ; but that, even if we whole-heartedly accept it in spite of all its difficulties, we do but make a little circle of light by pushing back the greater problems into the outer darkness, where, though we may forget them, they nevertheless surround us on every hand, perhaps pressing more nearly upon us than is recognised by modern Materialism.

* Review of Loeb's "Mechanistic Conception of Life," *Nature*, November 21st, 1912.

MIMICRY, MUTATION AND MENDELISM

By Professor E. B. Poulton, F.R.S.

IN the first number of *BEDROCK* the origin and growth of a mimetic resemblance was considered in relation to the theories of Charles Darwin and Henri Bergson. I now propose to describe and illustrate further recent discoveries in the same subject, and to discuss their bearing upon the place of Mutation and of Mendelism in evolution. A brief account of the theories of mimicry was given in the article already referred to. On the present occasion it is only necessary to point out that, although the growth of a mimetic pattern on the wings of a butterfly is a very short and a very late chapter of evolutionary history, the record is, within its limits, remarkably complete. The mutationist believes that evolution proceeds discontinuously by large steps. A fully formed mimetic pattern may certainly strike the observer as a large step, but its significance is magnified by the nature of the appeal that is made to us by the sense of sight. Mutationists and Mendelians have sometimes shown a tendency to yield to this appeal, and to measure the evolutionary importance of a change by the depth of a subjective impression. No effect caused by the presence or absence or the distribution of certain superficial colours can be compared for importance with changes involving such systems as the nervous, muscular, and skeletal. If it were possible to prove that a mimetic pattern arose fully formed and complete by a sudden mutation, it would by no means follow that more deeply-seated changes have been brought about in the same way. If, on the other hand, it can be shown that a likeness was evolved by the progressive modification of a series of stages, strong grounds will be afforded for the belief that more fundamental changes were effected gradually and not suddenly.

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I propose to consider two important examples of mimicry in African butterflies—examples on which much new light has been shed by recent researches. The models for mimicry belong, with a single exception, to the *Danainæ*: the exception is an *Acræine*. The *Danainæ* and *Acræinæ* have been shown by many experiments to be distasteful to insect-eating animals, and both are extensively mimicked in other parts of the world as well as in Africa.

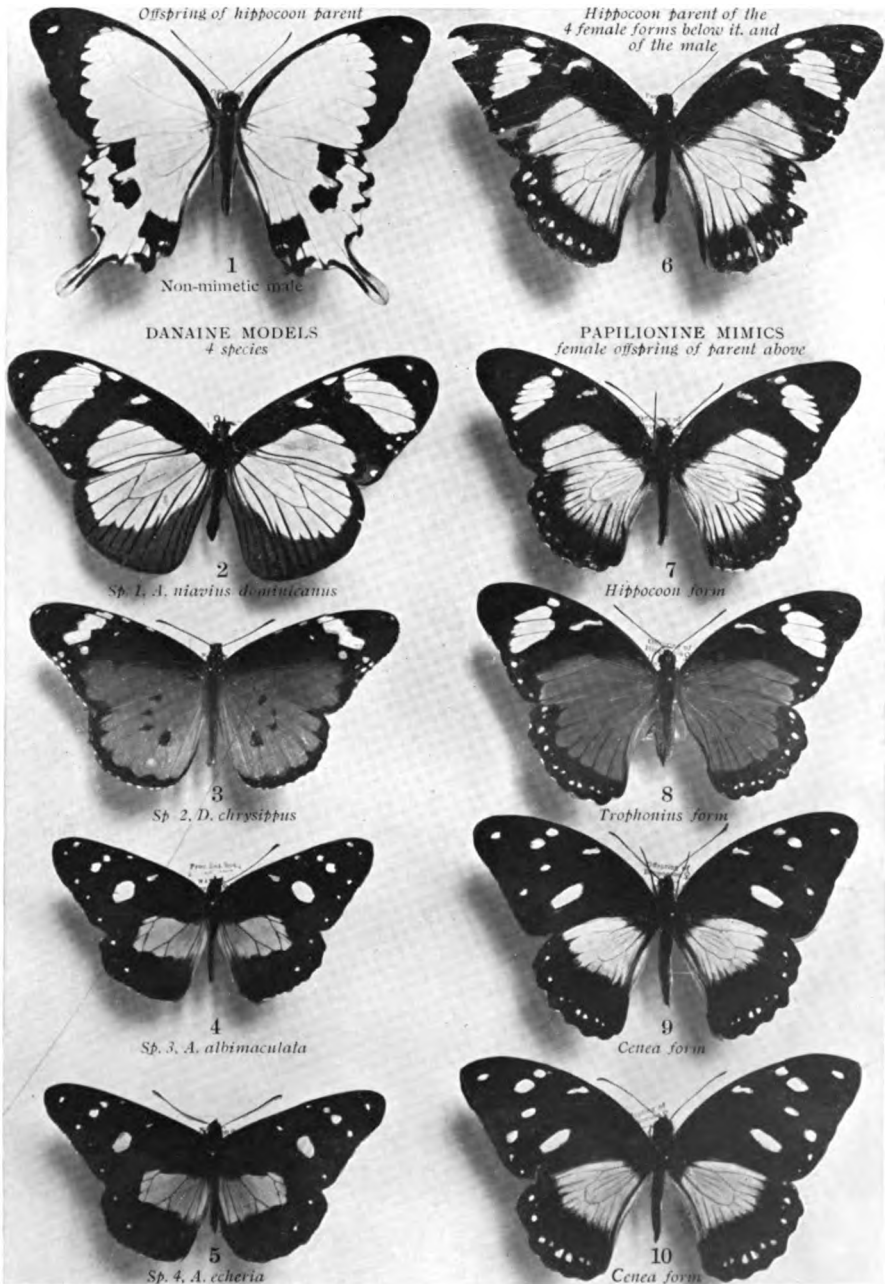
First, *Papilio dardanus*, or, as it used to be called, *Papilio merope*. The train of mimetic females accompanying the non-mimetic male of this species and changing in relation to the models in various parts of Africa has often been spoken of as the most wonderful example of mimicry in the world. The splendid conclusions announced by Roland Trimen in 1868 are clearly shown in the accompanying Plate I. When Trimen began his enquiries the butterfly shown in Fig. 1 was known as *Papilio merope*, that in Figs. 6 and 7 as *P. hippocoon*, in Fig. 8 as *P. trophonius*, in Figs. 9 and 10 as *P. cenea*. All these were considered to be entirely distinct species. After studying all the material available in museums and private collections in Africa and Europe, Trimen found that *merope* was invariably a male and the other three invariably females. By a masterly analysis of the markings of the three female forms and their varieties he brought out the essential resemblance that underlay the superficial divergence; while, by comparison with the non-mimetic female of an allied species in Madagascar (Plate II., Figs. 1 and 2), he was able to suggest the origin of the female forms from a pattern closely similar to that of the male.

All this evidence was discussed in Trimen's great memoir in the *Transactions of the Linnean Society* for 1869 (Part III. of Vol. XXVI., p. 497), in which he established the conclusion that *hippocoon* (Fig. 7 on the accompanying Plate I.) is a female form of *merope* (Fig. 1) modified by mimicry of the conspicuous Danaine *Amauris dominicanus* (Fig. 2), that another female form, *trophonius* (Fig. 8), arose in mimicry of *Danaida chrysippus* (Fig. 3), and a third, *cenea* (Figs. 9 and 10), in mimicry of *Amauris echeria* (Fig. 5). These views at first met with opposition and even ridicule, but confirmation of various kinds rapidly accumulated, and Trimen's conclusions were generally accepted long before the final proof was obtained at Durban in 1902, when G. F. Leigh bred eighteen males (*merope*),

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twenty-four *cenea* females and three *hippocoon* females from the eggs laid by a female parent of the *cenea* form which he captured in copula with a male *merope*. The still more wonderful family illustrated on Plate I. was bred by him in 1906 from a *hippocoon* form of female (Fig. 6). Of the twenty-eight offspring reared from her eggs, fourteen were males (Fig. 1), three were *hippocoon* females (Fig. 7) like the parent, three were *trophonius* females (Fig. 8), three were *cenea* females with white spots in the fore wing (Fig. 9), and five were *cenea* females with one or more of the spots yellowish (Fig. 10). The Hope Collection at Oxford now possesses seven families, bred between 1902 and 1910 by Mr. Leigh, from females captured in the neighbourhood of Durban—twice from *cenea*, twice from *hippocoon*, and three times from *trophonius*. A very striking fact was the predominance of *cenea* in the offspring of all seven parents. One *hippocoon* and one *trophonius* produced nothing but *cenea*. The whole of the offspring added together give ninety-eight males, ninety-one *cenea* females, nine *hippocoon*, eight *trophonius*, and two of a new female form, *leighi*, both of which appeared in the last family bred in 1910 from a *trophonius* female. This remarkable family also contained twenty-five males, twenty-two *cenea* females, two *hippocoon*, and four *trophonius*. I know of only a single Natal family of *P. dardanus* in which *cenea* is not the dominant female form—a brood, reared by Miss Fountaine from the eggs of *trophonius*, with nineteen females of the same form as the parent and two of the *cenea* form. Specimens strictly intermediate between the female forms have not occurred in any family that I have seen, but slight indications of transition between *cenea* and the other females are not uncommon. The parental form may apparently exert an influence on the colour of offspring belonging to a different form. Thus the hind wing patch of some of the *cenea* offspring is apt to be deeper in tint when the female parent was *trophonius* with its rich fulvous markings, than when they have been bred from the white-marked *hippocoon*.

The facts summarised above are consistent with, and indeed strongly suggest, a Mendelian interpretation of the hereditary relationships, but the complete and detailed proof would be very difficult to obtain because of the unknown tendencies borne by the male. This difficulty could probably be overcome by bringing eggs,



Alfred Robinson, photo.

Nearly $\frac{2}{3}$ of the natural size.

Andre & Seigh, Ltd.

Papilio dardanus cenea, the S. E. African Sub-species of *P. dardanus* with the four Danaine models of its female forms. The proof by breeding that the mimics are one species. (Near Durban, Natal, 1906, G. F. Leigh.)

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larvæ or pupæ from Gazaland in south-east Rhodesia and pairing the resulting males with Natal females. The *hippocoon* form is far more dominant in the former locality than *cenea* in Natal, and it may be safely assumed that the vast majority of the males would bear the tendencies of *hippocoon* alone. If the proportions of the female forms observed in any locality are reflected with tolerable accuracy in the families reared from females of that locality—and this is certainly true in Natal and in the Lagos district of West Africa—we may feel confident that nine out of ten *hippocoon* females from Chirinda in south-east Rhodesia would yield *hippocoon* females and no others. The experiment has not yet been attempted at Chirinda, but it has been tried in the Lagos district, where the western form of *hippocoon* is at least equally dominant. I here predicted that no females but *hippocoon* would be bred from the great majority of parents of this form. My kind friend, Mr. W. A. Lamborn, has now bred six families for me. The female offspring of all six are without exception *hippocoon*.

The explanation of the relative proportions of the female forms in different parts of Africa is to be found in the prevalent local *Danaine* butterflies and in the presence or absence of a single *Acræine*. In Natal, the two species of *Amauris* (Figs. 4 and 5 on Plate I.) are by far the most abundant *Danaines*, while *A. dominicanus* (Fig. 2) is generally rare, and often not to be seen at all. *D. chrysippus* (Fig. 3) is always common, but it frequents the more open woodland spaces, while *P. dardanus* prefers the dense forest; and model and mimic only mingle freely where the two types of country pass into each other. Probably on this account the *trophonius* form, although occurring wherever *dardanus* exists in Africa, is always relatively rare, while its ubiquitous model is common throughout the Ethiopian Region. As we pass westward into Cape Colony the proportions of the female forms remain much the same, except that *hippocoon* is even rarer than in Natal, while its model, *A. dominicanus*, is altogether unknown. Passing northward along the east coast, the striking feature is the rapid increase and predominance of *hippocoon* and the relative rarity of *cenea* as well as *trophonius*. This change corresponds with the rise in importance of *A. dominicanus*. Even where *echeria* and its ally are far more abundant than *dominicanus*, if the latter be at all common, *hippocoon* will be abundant and *cenea*

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rare among the female forms of *dardanus*. The explanation is almost certainly to be found in the conspicuous black-and-white pattern which makes *dominicanus* one of the most striking and easily remembered of all African butterflies. The comparison of Fig. 2 with 4 and 5, will at once suggest that *dominicanus* forms a far more striking feature in a forest than a much larger number of *echeria* and *albimaculata*.

With probable exceptions here and there in special localities where *dominicanus* is wanting or rare, the proportions of the three forms remain about the same up the east coast into British East Africa, and westward into the Uganda Protectorate. On the eastern shores of the Victoria Nyanza, however, *A. dominicanus* meets and becomes transitional into the western species *A. niavius*, with a smaller white patch (Plate III., Fig. 1), and west of the great lake we find *hippocoon* with a correspondingly reduced patch. *A. echeria* and its ally are very abundant in the Uganda forests, but somewhere west of the Protectorate they disappear, and when the coast is reached, and no doubt far into the interior as well, the *cenea* form is unknown, although *hippocoon* remains abundant and *trophonius* rare.

A new female form appears on the east of the Victoria Nyanza, becomes fairly common on the west of the lake, where it is probably next to *hippocoon* in abundance, appears on the west coast in Angola, and almost certainly occurs over the intervening area. This is the *planemoides* form recently described by Trimen, and it is of great interest inasmuch as its models are *Acraëne* and not *Danaine*. One of its two models, the male of *Planema macarista*, was represented in Fig. 6 on the plate facing p. 58 of the first number of BEDROCK (April, 1912). The male and female of its second model *Pl. poggei* are so similar to the male of *macarista* that the same figure gives a good idea of their general appearance. It is interesting to note in passing that these *Planemas*, with their striking pattern, were selected for illustration and description in the earlier article because of the influence which they exert upon the females of an *Acraë*, viz., upon a mimic remote from the *Papilioninæ* to which *dardanus* belongs. Equally clear examples of their influence in still other groups could be described if space permitted. Any hypothesis which aims to interpret the phenomena of mimicry must take into account the fact that the influence of a striking dominant model commonly

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radiates into a whole circle of mimics belonging to a series of remote groups.

The *planemoides* female of *dardanus*, with a broad fulvous bar crossing the fore wing, and a large white patch covering the base of the hind, stands out as very distinct from the other three mimetic forms. *Planemoides* has not yet been proved by breeding to be a female form of *dardanus*,* but evidence equally strong is fortunately provided by a single specimen captured by Captain T. T. Behrens (1902—3) in Buddu, on the west shore of the Victoria Nyanza. In this specimen the pale yellow scales and black markings of the male replace the female pattern on parts of both wings on the left side. The evidence of specific identity is certainly curious and interesting, but it is conclusive. Such a fusion of characters can only occur between the male and female of the same species.

It is, as I have said, very probable that the relationship between the female forms of *dardanus* is Mendelian, and that the establishment of mimicry in various parts of the range of the species has been greatly facilitated by the fact that the female forms keep true, and do not commonly produce intermediates. Furthermore, in certain other polymorphic mimics, the Mendelian relationship may be accepted as proved. But this acknowledgment of the debt which polymorphic mimicry owes to Mendelian heredity by no means implies acceptance of the view advocated by some Mendelian writers—in particular Professor Punnett—that each mimetic pattern arose, suddenly and complete, as a mutation from the non-mimetic ancestor. To suppose that each of the forms represented in Plate I., Figs. 7, 8, 9 and 10, sprang suddenly into existence from some ancestral non-mimetic female resembling that of *P. meriones* in Madagascar (Plate II., Fig. 2)—that each of them, without adaptive adjustment, at once matched the patterns of the four

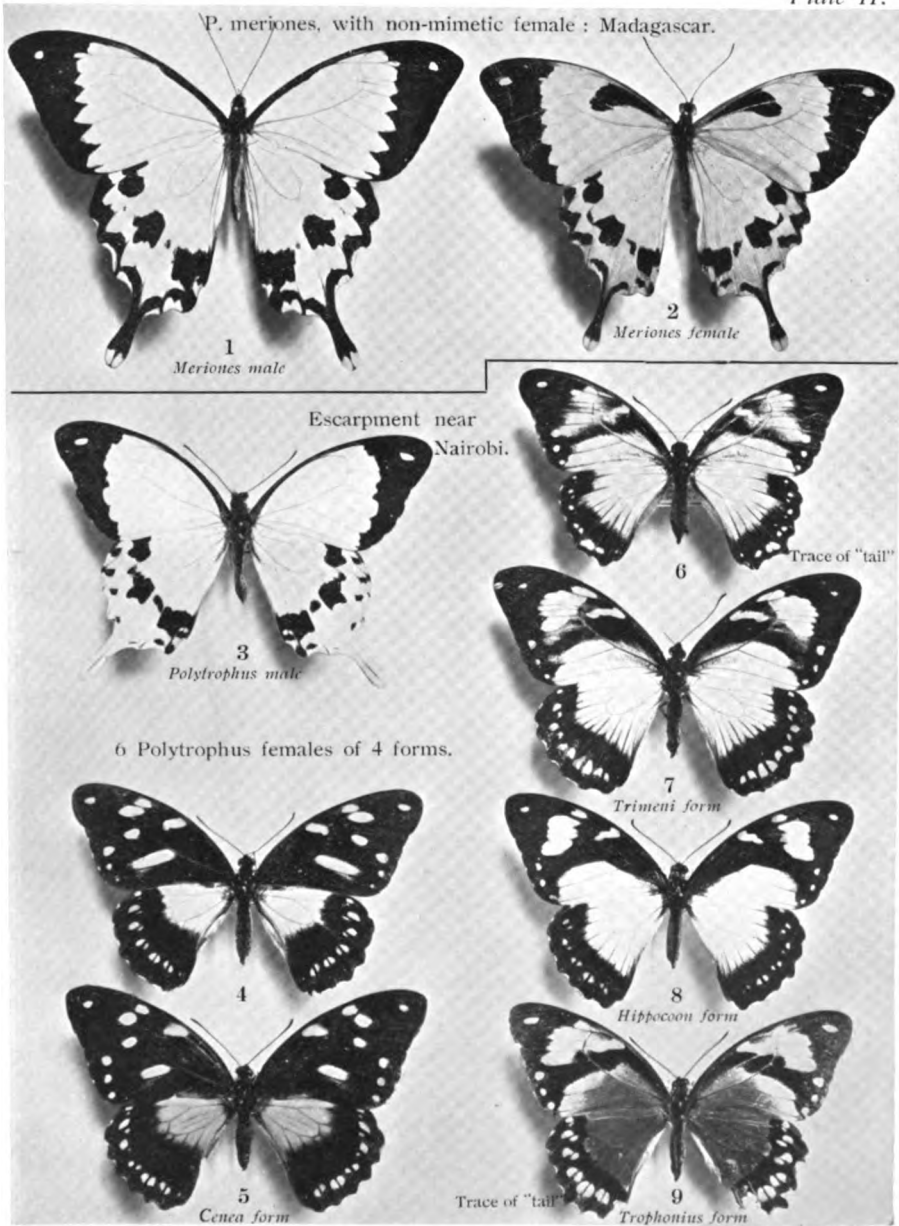
* Since these words were written, my kind friend, Dr. G. D. H. Carpenter, has obtained twenty-six eggs from a *planemoides* female on Bugalla, one of the Sesse Islands in the north-west of the Victoria Nyanza. In his last letter I heard that twenty-five caterpillars were thriving and had changed their third skins. We may anticipate that the female offspring will be chiefly or entirely *planemoides* and *hippocoon*.

March 7, 1913. As I correct these proofs I am able to add the result of this most interesting and long-sought-for experiment in breeding. In a letter received this morning, Dr. Carpenter tells me that three female offspring are *planemoides* and seven *hippocoon*.

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Danaines shown in Plate I., Figs. 2, 3, 4, and 5, is an astounding hypothesis, and one which could never have been advanced by a writer who had studied all that is known of the *dardanus* group. A large amount of evidence has been simply ignored by Professor Punnett, and I can only assume that he is unaware of its existence. It will appear in the succeeding paragraphs that the past history of these mimetic forms can be reconstructed with singular completeness from essential phases of the past which still survive in certain parts of the vast range of the *dardanus* group.

Trimen, in his original paper, pointed out that *Papilio meriones* of Madagascar (Plate II., Figs. 1 and 2) gives us a picture of the ancestral non-mimetic form, and he suggested that the black marking on the front, or, as it is called, the costal margin of the fore wing of the female (Fig. 2), was the origin of the bar which is the characteristic feature of the *hippocoön* form. A little later another non-mimetic species, *P. humbloti*, was discovered in the Comoro Islands, and a third, *P. antinorii*, in Abyssinia and Somaliland. In both of these the female bears a black costal marking corresponding with that of the female *meriones*. The essential discovery of forms linking the above three non-mimetic females with *hippocoön*, the most ancestral of the mimics, was due to the fine collection made in 1900 on the Kikuyu Escarpment, near Nairobi, by the late W. Doherty. Here, on the heights forming the eastern boundary of the Rift Valley, the most interesting series of ancestral females has been preserved. Chief among them is the *trimeni* form (Plate II., Figs. 6 and 7), which preserves for us just the stage predicted by Trimen. In some specimens (Fig. 6) the bar crossing the fore wing is barely complete, in others (Fig. 7) it is nearly as fully formed as in *hippocoön* (Fig. 8). The colour is still pale yellow, although, as in *meriones* (Fig. 2), dingier than that of the male. But perhaps the most interesting ancestral feature is the retention in some specimens of *trimeni* (Fig. 6) of a rudimentary "tail" to the hind wing, and it is significant that *hippocoön* is the only mimetic form which I have hitherto been able to find with rudimentary "tails." Such vestiges are to be seen in two examples of *hippocoön* in the British Museum, and they appeared in two specimens of the family first bred by Mr. Lamborn. I then suggested that he should try the effect of ice upon the pupæ of a family. The experiment was very difficult to



Alfred Robinson, photo.

Rather over half the natural size.

Andre & Sleigh, Ltd.

The non-mimetic ancestor of *Papilio dardanus* (*merope*) from Madagascar, and transitional forms, shewing the origin of mimetic females, from the Kikuyu Escarpment, near Nairobi, British East Africa (6,500—9,000 ft.).

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carry out in a tropical station, seventy miles from Lagos, but Mr. Lamborn was able to maintain a temperature of about 50° F. for rather over three days. Four out of the fourteen resulting females possessed rudimentary "tails," so it is probable that some effect was produced by the shock. The *trimeni* female also occurs, although very rarely, on the East coast, and an ancestral form resembling it in the imperfect bar crossing the fore wing has long been known as *dionysus* on the West coast, where it is very rare as compared with *hippocoon*. The Kikuyu Escarpment is the only locality at present known where these transitional forms make up a large proportion of the females. Side by side with them the fully developed *hippocoon* (Fig. 8) occurs together with all the other forms, including *planemoides*. This locality is also unique in the numbers of unnamed varieties and transitional forms. The origin of *trophonius* (Plate I., Fig. 8) is well seen in the form shown in Plate II., Fig. 9—a *trimeni* female with yellowish markings and even a slight trace of the "tail," but with the great patch extending over a large part of both wings almost entirely overspread with a fulvous flush. The *cenea* female—the most specialised of all—was also probably evolved from *trimeni*; for the specimen represented on Plate II., Fig. 4, although possessing the fully developed pattern (compare Fig. 5, as also Plate I., Figs. 9 and 10), still retains the ancestral pale yellow markings. Furthermore, most of the markings in the fore wing are recognisable, although with indistinct outlines, in the fore wing of some examples of *trimeni* (compare Figs. 4 and 6 on Plate II).

The *planemoides* form probably arose in association with the origin of *cenea*, the hind wing patch becoming white, while the reduced pale markings of the *trimeni* fore wing, instead of concentrating into spots, broke through the black bar and, gaining a rich fulvous tint, fused into a broad band crossing the wing. A single example of *planemoides* obtained by the Rev. St. Aubyn Rogers in the Mombasa district, hundreds of miles east of its *Planema* models, exhibits ancestral features in the tendency of the fulvous band to divide along the line of the original black bar. In the *leighi* form which has occurred several times in Natal—twice in a single family as described on p. 44—and thus at an immense distance from the tropical model, we meet with another still more ancestral stage of

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the *planemoides* female in which the fulvous band is represented by three widely separated patches corresponding in position and form with the two chief costal markings of *hippocoon* or *trophonius* and the largest oval marking in *cenea*. It is impossible to interpret *leighi* as a hybrid between one of the other female forms and *planemoides*, because the latter is entirely unknown in Natal and indeed far to the north of it.

Further important evidence in favour of the gradual building up of the mimetic forms is furnished by a careful study of the thirteen families of known parentage in the Hope Collection. We thus learn that small features in the pattern of the parent certainly tend to reappear in her offspring. I have traced this in three markings or sets of markings, but will here confine myself to one. It has been pointed out that the principal marking of the *hippocoon* female of the east (Plate I., Fig. 7 ; Plate II., Fig. 8) is very large, like that of its model (Plate I., Fig. 2), but that the same marking is much smaller in the west, corresponding with that of the western *Danaine* (Plate III., Fig. 1). Now Mr. Lamborn's western families of *hippocoon* females exhibit marked differences in the size of the marking, so that the majority of the females of one family are a small but distinct step nearer to the *hippocoon* of the east than is any one of the females of another family. Hereditary material exists which, given selection, could easily produce the eastern from the western mimic, or *vice versa*. Furthermore, transitional forms are common near the zone where the one model passes into the other. It is difficult to see how the evidence of an evolution by gradual steps could be stronger than it is.

The second example is not quite so complex, and it affords a very interesting comparison with *dardanus*, inasmuch as both males and females are mimetic. The important Oriental and Ethiopian genus *Hypolimnas* belongs to the *Nymphalinae*,—the great group of butterflies which includes our English Purple Emperor, White Admiral, and *Vanessa* and its allies, including the Red Admiral, the Peacock, the Tortoiseshells and the Comma. *Hypolimnas* is nearly related to these butterflies, and the chrysalis of the species we are considering (*H. dubia* and *anthedon*) closely resembles that of our common *Vanessids*. The larva too is black and spine-covered, and feeds on a kind of nettle (*Fleurya*), like many *Vanessas*. Nearly the whole

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genus *Hypolimnas* is mimetic in one or both sexes, although this is not true of *dexithea*, an extraordinary ancestral species of huge size in Madagascar. The models are chiefly, in Africa almost exclusively, *Danainæ*. A group of African species, with both sexes mimetic and both generally alike, is sometimes separated as a distinct genus, *Euralia*, from *Hypolimnas*, in which the sexes are generally unlike and mimicry is confined to the female; but the distinction breaks down on both sides.

One of the commonest East African species of the group with both sexes alike, *Hypolimnas* (*Euralia*) *wahlbergi*, resembles *Amauris dominicanus* (Plate I., Fig. 2), while *H. (E.) mima*, a second species, as it was regarded till quite recently, mimics the two species of *Amauris* represented in Figs. 4 and 5 of the same plate. Just over ten years ago Mr. Guy A. K. Marshall published his conviction that *wahlbergi* and *mima* were a single species.* He pointed to the facts that the two forms were known to pair, that intermediates between them were known, and that he had observed them going to rest together in the evening as the individuals of some species are known to do. From that time I endeavoured to persuade African naturalists to breed the species and test Mr. Marshall's hypothesis. For many years these efforts were unavailing, one chief difficulty being the ignorance of the early stages and the larval food-plant. At length, in 1909 Mr. A. D. Millar—a distinguished Durban naturalist, whose recent death is a severe blow to African zoology—discovered the food-plant and bred both forms from the eggs laid by a female *wahlbergi* and later by a female *mima*.† His results suggested, although they did not prove, that *mima* was a Mendelian dominant, and *wahlbergi* recessive. There can be little doubt, as in *P. dardanus*, that the mimic of *dominicanus* is ancestral as compared with that of *echeria* and its ally. If we consider the genus *Hypolimnas* as a whole, especially the Madagascar species and the males that have not been modified by mimicry, we are led to conclude that the pattern of *wahlbergi* is nearer to the general type than *mima*. This affinity is especially indicated by the prevalent blue scales on the border of the white patches. Professor Punnett has discussed *wahlbergi* and *mima* and their models on pp. 134—5 of the latest

* *Trans. Ent. Soc.*, 1902, pp. 491—2.

† *Proc. Ent. Soc.*, 1910, pp. xiv—xvi.

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edition (1911) of *Mendelism*, where he makes the following extraordinary statement :—

“ On the modern Darwinian view certain individuals of *A. dominicanus* gradually diverged from the *dominicanus* type and eventually reached the *echeria* type, though why this should have happened does not appear to be clear. At the same time those specimens [of *Hypolimnas* or *Euralia*] which tended to vary in the direction of *A. echeria* in places where this species was more abundant than *A. dominicanus*, were encouraged by natural selection, and under its guiding hand *mima* eventually arose from *wahlbergi*.

“ According to Mendelian views, on the other hand, *A. echeria* arose suddenly from *A. dominicanus* (or *vice versa*), and similarly *mima* arose suddenly from *wahlbergi*. . . . On this view the genera *Amauris* and *Euralia* contain a similar set of pattern factors, and the conditions, whatever they may be, which bring about mutation in the former lead to the production of a similar mutation in the latter.”

Professor Punnett's conception of the origin of mimetic resemblance as set forth in the last-quoted sentence, has already been spoken of as a satisfactory substitute for the Darwinian interpretation, by Mr. Francis B. Sumner.* It amounts to this. A complex pattern A arose, we know not why or how, from another complex pattern B in a *Danaine* butterfly, while at the same time *a* (resembling A) arose from *b* (resembling B) in a butterfly of a widely removed sub-family. Although model and mimic are in every other respect widely different, in this one single but highly complex feature of pattern they are, according to Professor Punnett, identical. He tells us that the same conditions, whatever they may be, acting upon the same factors, produced the same result in these utterly different butterflies. But it is only necessary to look at the pattern of the mimic with the lens, or even critically with the naked eye, to see that in every one of its elements, it is not the same, but widely different from the model. The scaling is different, the quality of the colouring is different, the outlines of the pattern can be seen, even in the reduced reproduction of the corresponding western forms shown in Plate III., Figs. 1 and 2, to be wholly different—those of the model hard and sharp, those of the mimic soft and transitional into the dark ground-colour. If Professor Punnett's

* *The Journal of Philosophy*, New York, Vol. IX., pp. 159—61.

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statement were correct, his explanation amounts to this—"It is so, because it is so." But the statement is incorrect; the patterns only *appear* to be similar, and the problem to be solved is the production of so striking a resemblance not out of the same, but out of very different elements,—the fact that model and mimic are

"Not like to like, but like in difference."

We now come to a still more obvious objection. Darwinians, according to Professor Punnett, believe that one Danaine model arose directly although gradually, from the other—Mendelians, that the origin was sudden. To suggest either the one or the other is to show a want of acquaintance with the genus *Amauris* to which both models belong. The two butterflies *A. dominicanus* and *A. echeria* are widely separated. Aurivillius in his great "*Rhopalocera Æthiopica*," places *dominicanus* (considering it to be a form of *niavius*) as the second, *echeria* as the fifteenth species of *Amauris*, and no systematist has suggested a nearer affinity. Dr. F. Moore, in fact, in his revision of the *Danainæ*,* placed *echeria* and an allied species in a separate genus, *Nebroda*. It would be interesting to know whether Professor Punnett applies his hypothesis consistently and believes that the Acræine model of the *planemoides* female of *dardanus* arose directly from one of the Danaine models (or *vice versa*)!

In each of the examples of mimicry considered in the present article we have to deal with distinct species of models resembled not by distinct species of mimics, but by the polymorphic forms of a single species. This is one of the most interesting aspects of the question; for, as Dr. Karl Jordan has argued, there are no grounds for the belief that these polymorphic mimetic forms are on their way towards the formation of separate species. Polymorphism itself has become a character of the species, just as it has in the well-known *Kallimas* with their various types of dead-leaf-like under surface. It is, however, a character that is only kept up by constant selection. We have seen in *dardanus* that the absence of the model leads to the absence or the extreme rarity of the corresponding female form. And the same is true of the Nymphaline mimic; for

* *Proc. Zool. Soc., Lond.*, 1883, p. 201.

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my friend, Rev. K. St. Aubyn Rogers, who has done so much for the Oxford University collections, informs me that in parts of the Mombasa coast district, where *Amauris dominicanus* occurs, but *echeria* and its ally are wanting, the *wahlbergi* form is common, but *mima* never seen. Moreover, about a hundred miles to the west the latter model again becomes common and *mima* at once reappears.

When Mr. A. D. Millar proved that *wahlbergi* and *mima* were dimorphic forms of one species, it became obvious, as I had suggested in 1902,* that their respective Western representatives *anthedon* and *dubia* are also mimetic forms of one species. Furthermore, in Uganda these Eastern and Western forms meet, and there can be little doubt, as Dr. Karl Jordan has pointed out, that we are concerned with a vast continuous interbreeding community,—a single species with corresponding forms modified by mimicry on the opposite sides of the Continent. The far-reaching interest of this conclusion made it all the more important to test the western forms by breeding. A set of these mimics (Figs. 6—10) with their models (Figs. 1—5) captured by Mr. Lamborn in the neighbourhood of Oni Camp seventy miles East of Lagos is shown on Plate III. *Anthedon* (Fig. 6), the western form of *wahlbergi*, is the mimic of *niavius* (Fig. 1), the western form of *dominicanus* (Plate I., Fig. 2): *dubia*, on the other hand, although obviously corresponding with *mima*, appears in three forms (Figs. 8, 9 and 10) mimicking three species of *Danainæ* (Figs. 2—5). The first form (Fig. 8), with a brown shade bordering the white patch of the hind wing, mimics *Amauris egialea* (Fig. 2); the second, more strongly marked with white, mimics the commoner form of *Am. psyttalea* (Fig. 3): the third, with the white markings reduced, especially in the hind wing, mimics *Am. hecate* (Fig. 5) and a rarer form of *Am. psyttalea* (Fig. 4), which is itself a mimic of *hecate*.

In 1911 and 1912 Mr. W. A. Lamborn bred twenty families, containing over 1,400 offspring, from known female parents, some of them *anthedon* and others including all the above-mentioned forms of *dubia*. *Anthedon* behaves as a recessive, four times producing all-*anthedon*, once all-*dubia*, and three times mixed families as it would do if it had been mated with recessives, a

* *Trans. Ent. Soc.*, p. 492.



Danaine models of four species mimicked by four forms of a single Nymphaline species. Proof by breeding that the mimics are one species, and that a parent if slightly intermediate produces slightly intermediate offspring. Lagos district of S. Nigeria. (W. A. Lamborn, 1910-12.)

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dominant, or heterozygotes respectively. *Dubia*, in Mr. Lamborn's experience of twelve parents, always gave mixed families. A very striking result was the strong tendency of the *dubia* offspring in these mixed families to belong to the same form as the female parent. The proportions of the mixed families are sometimes in accordance with Mendelian expectation, sometimes rather strongly opposed to it. The results, as a whole, render it improbable that these exceptions are to be accounted for by the same female pairing with different males. A great difficulty in the way of the usual Mendelian interpretation is the fact that *anthedon* and the *dubia* forms predominate at different seasons. Mr. Lamborn has observed this in the field, and it is also shown in the material I have received from him.

Returning to the above-mentioned four Danaine models of the Lagos district, it would be interesting to know whether Professor Punnett considers all these four Danaine models to be a little self-contained group of the genus *Amauris* produced by direct mutation within its own limits. Any such assumption would be quite unjustifiable. *Am. niavius* stands quite apart from the others, just as its eastern form, *dominicanus*, does from *echeria*; and among the three other species the resemblance of the form *psytalea* (Fig. 4) to *hecate* (Fig. 5) is purely superficial and no indication of affinity. This statement can be verified even from the reduced figures of the plate. The males of Danaine butterflies almost invariably possess a scent-organ which is believed to be used as an attraction to the opposite sex during courtship. In the genus *Amauris* this scent-organ takes the form of a double tuft at the extremity of the body and a patch near the anal angle of the wing—the corner opposite to the end of the body. These patches are well shown on the left side of Figs. 2—5, and it will be seen at once that both forms of *psytalea* (Figs. 3 and 4) have a similar patch, small in size and dead black in appearance, while the distinctly double patch of *hecate* (Fig. 5) is long and glistening like the shorter one of *egialea* (Fig. 2), each half of which is seen to be distinctly concave from side to side. No resemblance in the scent-patch accompanies the likeness of the pattern of Fig. 4 to Fig. 5. There is no real approach, but only a misleading superficial resemblance. The objection to these scent-patches as criteria of specific distinction is the fact that they

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are confined to one sex ; but, as regards the males, they afford a most excellent test. Furthermore, Mr. Lamborn has bred two families of *Am. psyttalea*, and in both the butterflies were nothing but *psyttalea*.

The pattern of *niavius* (Fig. 1) and its mimic *anthedon* (Fig. 6) stands widely apart from those of the other models and mimics represented on Plate III. The principal marking of the former spreads over both wings, but in the latter it is confined to the hind wing. Intermediates between *niavius* and the other models are unknown, but are not very uncommon between *anthedon* and the other mimics. A good example strongly on the *anthedon* side of intermediate is represented in Fig. 7, another strongly on the *dubia* side in Fig. 11, and its intermediate offspring in Figs. 12, 13 and 14. The pattern is continued on to the fore wing in these last four examples as a brown, and not as a white streak, and even in the original of Fig. 7 the corresponding part of the fore wing is of a pale brownish shade. Mr. Lamborn has twice bred from a female with the pattern shown in Fig. 11. The entire smaller family is represented in Figs. 12—16, and in both it and the other far larger family the *dubia* offspring strongly inherited the intermediate tendency of the parental pattern. The intermediate pattern behaved in heredity as a definite whole, and did not split up as though it were a hybrid (or heterozygote). It is reasonable to conclude that such specimens as those shown in Figs. 7 and 11 are, like *trimeni*, *leighi* and *dionysus*, relatively rare but persistent ancestral steps showing us how, under the guidance of selection, the wide gap between the pattern of *anthedon* and that of *dubia* has been crossed.

It may be safely concluded from the consideration of these two interesting and complicated examples of mimicry that the mimetic resemblance has not been produced from the original non-mimetic form by a sudden mutation, but has been the result of a series of transitional steps, some of which are still preserved.

ON TELEPATHY AS A FACT OF EXPERIENCE: A Reply to Sir Ray Lankester

By Sir Oliver Lodge, F.R.S.

"If ye expect not the unexpected ye shall not find truth."—HERACLITUS.

IN my article in the October issue of *BEDROCK*, I spoke of the question "whether consciousness apart from brain has any meaning" as the fundamental issue; and this sentence has been used, quite justifiably, by Sir Ray Lankester in the title of his reply. But this important and profoundly difficult question is not the immediate phenomenon that the Society for Psychical Research has been investigating, nor is it the phenomenon under discussion. The possible existence of consciousness apart from brain (however obviously consciousness requires brain to manifest itself here and now) is not a phenomenon at all, but a hypothesis—one that I spoke of as eminently debatable; though I admitted that it was the working hypothesis to which I have been myself led by long continued study of a considerable range of obscure psychical facts. Such a hypothesis does not rest solely on the occurrence of simple telepathy, on which the present discussion hinges: it is sustained by a good deal more.

The experimentally observed fact immediately under consideration is the transference of thought or mental impression between a few living people, *i.e.*, between such as have the faculty sufficiently developed, without the use of their normal sense organs; the conditions of transfer being not yet known. But what the explanation of this fact may be is an open question. It may *possibly* be due to brain waves, or some kind of syntonistic material or ethereal connection between brains; for, though I think that unlikely, it is what some people have suggested and provisionally hold—finding in it an obvious analogy with wireless telegraphy; and if that can

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be proved to be the explanation, no question of consciousness apart from brain need arise : i.e., no such hypothesis would in that case be necessary to account for simple telepathy. The process would then take its place as an extension of, or addition to, the already known methods of transmitting thought—speech, writing, gesture, code signalling, etc. Some of these methods would seem mysterious to a savage, just as telepathy may seem mysterious to us so long as we are unacquainted with the mechanism of the process. On anything beyond affirming the bare fact I have been careful not to dogmatise ; though I myself am inclined to maintain that telepathy is not a physical process, *pari passu* with the other or long known methods of communication, but is a sign or incipient outcome of a faculty and a method essentially different.

As to evidence of the existence of such a faculty. If a *record* is wanted, it is contained in a mass of papers published in the *Proceedings* of the Society for Psychical Research, for it serves as the hypothetical and most nearly orthodox explanation of a multiplicity of phenomena which without it would be even more mysterious : it is the minimum hypothesis, which we feel bound to stretch to the utmost before going beyond it. If, however, direct first-hand laboratory *experience* of the rudimentary stages of such a faculty is wanted—as it ought to be—it must be looked and waited for, and experiments must be tried from time to time, as in any other branch of science. An opportunity for investigation with a plausible chance of success is likely to occur if observer are patient, though it is not always at hand ; and my experience shows that sooner or later sufficiently sensitive or capable percipients—persons with adequate receptivity—will be found. Perhaps they are commoner than we think, because the test is so seldom applied. I have now an apparatus set up for examining whether traces of the faculty exist widespread in normal people ; and I shall make report to the S.P.R. in due course. But whether incipiently widespread or not, and however it be explained, the faculty of telepathic receptivity certainly exists in a few people ; though even in them it is by no means always and under all circumstances available. In that respect it differs from an inorganic property like radio-activity ; though that, too, appears limited to a few substances and is not conspicuous or widespread. Twenty years

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ago, indeed, it might have been denied. I fully expect that, even at a date so recent, many authoritative people would have declared a perennial emission of energy by an inert substance, without apparent supply, to be simply impossible. Testimony to any completely new phenomenon is likely to sound incredible at first hearing.

The circumstance, however, to which Sir Ray Lankester chooses to direct his attention, and which he discusses in his recent article, is not a question of telepathy at all, and needs no troublesome inquiry ; it is merely the question of how it happens that moderately sensible people can believe in such an "impossible" occurrence or class of occurrences ; or, as he expresses it,

"how does it come about that there are individuals . . . who assert their belief in telepathy, and even assert that they have witnessed or experienced certain things which, if truly recorded in the statements made by them, would go far towards proving the existence of a communication of mind with mind at a distance and without the intervention of the known channels of the senses ? "

And to account for this "phenomenon," as he calls it, he tabulates eleven alternative hypotheses which might account for it, and which it may be convenient here to summarise in abbreviated form, thus :—

That individuals who hold such a belief are either

1. Liars
2. Fools
3. Prejudiced
4. Temporarily insane
5. Mad—at least to the extent of monomania
6. Physically defective
7. Feeble-minded
8. Obsessed by the marvellous
9. Ignorant
10. Deceived by coincidences
11. Impressed by the truth.

And he says that each one of the first ten of these hypotheses is more likely to be in correspondence with reality than is the eleventh supposition, namely, the actual existence of a mysterious communication between mind and mind.

This, he says,

"is the last hypothesis to which we should have recourse—only to be made use of when all the others have been proved to be inadequate."

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The other ten are well known and established as common facts, and therefore are to be preferred to No. 11. In fact, he finds, in actual practice, that one or other of the ten hypotheses does account for the statements in every case, and that the eleventh is not needed.

Well, of course, this is Hume's argument against miracles over again, though in rather a vigorous militant form. An investigator is naturally on his guard against weaknesses of this sort, and is far from accepting testimony from lunatics and credulous fools; but he must, if himself sane, be allowed to form an opinion concerning occurrences in his actual experience, else it would be extremely difficult—indeed quite impossible—ever to establish the existence of a previously unknown faculty. Sir Ray Lankester takes his stand on the assumption—the tacit major premiss of his argument—that the main outline or scheme of terrestrial existence, including certainly every human faculty, is already familiar and recognised by science: whereas we call his assumption gratuitous, and, from the vantage ground of positive experience, condemn his negation.

To this he will rejoin that when an individual throws doubt upon a general uniformity of experience, the discussion is bound to turn to some extent upon a personal question—the competence of the person who makes the assertion which runs counter to prevalent belief. And I agree that a scornful attitude is the one which we all naturally take up against flat-earth and perpetual-motion people. But it may be possible to distribute such a prejudice too widely, and extend its range too far: the exceptional experience may after all be real, since it has not, like squaring the circle, been proved impossible. The one advantage of prejudging the issue is that thereby all trouble is saved—the trouble of really going into the matter and obtaining and examining unlikely facts at first hand.

The business man takes another line and offers a thousand pounds for proofs which will convince him. He has, of course, no intention of parting with the money, and is quite satisfied that he can resist any temptation to be convinced. Nor, indeed, ought anyone to be convinced by a single instance—opportunities of error and oversight are far too plentiful—long experience is necessary, and trials under many varying conditions. A thousand-pound note is a weird argument in science, though to the offerer it possesses the one-sided advantage that the issue can thereby be brought into a law-

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court and decided in accordance with the present average state of knowledge, i.e., against anything unfamiliar. To all wagers of this kind I trust that those connected with the S.P.R. will always turn a deaf and contemptuous ear.

Sir Ray Lankester is so obsessed with the conviction of the impossibility of any discovery, of a fundamentally new kind, in psychology or psycho-biology (a tract of country as yet but little explored), that he prefers to think a large number of reputable people either lying or insane, rather than recognise any possible truth in their carefully recorded experiences and published assertions. It is clear that he considers the record of what we call the evidence, which now occupies twenty-five volumes of the *Proceedings* of the S.P.R., as merely the sign of somewhat widespread mental disease.

I do not know whether Sir Ray Lankester will ever be convinced of the truth of these things in any form—probably not. In that case there will be a gap in his encyclopædic knowledge. It is not my business to convince him. As a friend I might like to do so, but it would be an unwieldy task. I say “as a friend,” because I take it that we are not really unfriendly to each other, in spite of his ten hypotheses. He does not really apply them all to me. He is probably satisfied with a selection, and might choose, shall we say, 2, 7, 8, 9 and 10. On the other hand, when considering his unfortunate case, I find that Nos. 3 and 9, re-worded a little, may be taken as sufficient, without any of the more violent alternatives.

He seems to have had an unsatisfactory experience with a professional medium forty years ago, and he speaks of having been definitely refused the opportunity of examining phenomena by the Society for Psychical Research. I know nothing of that; but suppose we did try to introduce him against his will to an experimental case of the phenomenon, he would lack ingenuity if he could not find some way of inhibiting it. It appears that he was invited by Mr. Serjeant Cox, long ago, to see something; and the result was a police court prosecution, which presumably was not what his friend anticipated. He will say that Slade was a fraud. Well, apparently he was; but one experience with a trickster can hardly be regarded as a sound or scientific basis for a comprehensive induction regarding a whole class of phenomena. The S.P.R. has

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done more in the way of detecting fraud than any single person ; partly, no doubt, by reason of its wide experience ; but it has also had a large experience of genuine phenomena, and by application of the comparatively simple principle of telepathy to cases where its application is not superficially obvious, and where any other explanation involves a greater departure from normality, it has done a good deal to mitigate superstition, to give a rational explanation of at least one kind of so-called ghost, and generally to induce a sane and sober treatment of a number of uncommon but genuine experiences.

It is sometimes said—Sir Bryan Donkin either says or clearly implies—that the existence of telepathy would contradict established knowledge. If that were true, it would indeed be an absurdity, but telepathy does nothing of the kind ; it enlarges and expands, it opens up a new chapter, but it does not contradict. By psychical research our knowledge of fact is supplemented, but in no other way changed. The unwelcome facts will fit into the coherent scheme of science in due course, and will displace nothing already there, though they will remove some mistaken accretions—the beginnings of a premature fence or boundary. If biologists have formulated for themselves a theory that a material mode of access is the only access to mind, and material methods the only possible means of psychic inter-communion—that theory, well founded as it is on the positive side, may have to give way on the negative side, and the word “ only ” be eliminated from the statement before it is true. Subject to correction by further experience, I am ready provisionally to agree that possibly the only way in which mind can act on inorganic matter is through or by the aid of the brain nerve and muscle system of some living animal ; and conversely that the material Universe acts, and perhaps only can act, on mind by an inverse process through the sense organs of a living person. At any rate, there is great uniformity of experience in support of these commonplace theses. But if I am asked the totally different question whether it is likely that mind can ever directly act on mind without any material concomitant or intervention, I should have to say that I know no fact against it, and should wish to be simply agnostic on the subject were it not for the telepathic evidence which in recent times has come to our knowledge. And even then—

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for any interpretation or demonstration, for any *manifestation* of the occurrence—a physiological system of brain nerve and muscle must still be utilised ; since this is the essential condition by which we can obtain a record of the fact, or convey knowledge of it to the world. I said this, more briefly, but I thought clearly enough, in my former article.

I know that it annoys Sir Ray Lankester, and, I fear, the Editorial Board and readers of this Journal, to see the word “discovery” used of a thing which to them seems impossible ; namely, the direct intercommunication between mind and mind, apart from the operation of normal and recognised sense organs. But I know what the word “discovery” means in Science, and I take full responsibility for its use in this connection. I will, for their edification and amusement, gibbet myself further—since I have no wish to shirk expressing my own view with sufficient explicitness in a fair though hostile organ—so I go out of my way to say that I quite expect that the process which we call telepathy, whose laws I should be glad to understand, will be found applicable to, and will so to speak explain, or at any rate be closely connected with, those long-testified-to and frequently-encountered experiences, which simulate, and perhaps in some rare cases truly represent, communications from another order of mental existence which is normally dissociated from ordinary matter.

To sum up. Anyone who limits his range of enquiry to the general categories of already acquired knowledge has a sufficiently rich and extensive field, and, by surrounding himself with a definite boundary, is in a very strong position. Entrenched in such a fortress, Sir Ray Lankester, and those who think with him, look with pitying eyes on us who, after some exploration inside, have ventured outside the walls ; and they regard with contempt any assertions as to what lies beyond the pale. They are like the orthodox mariners of old who limited themselves to the shores of the Mediterranean, cruising round its coasts and gradually becoming familiar with every port. The world as known to the Ancients was their domain, and it was impious to sail out through the Pillars of Hercules into the ocean beyond. Venturesome explorers who transgressed those limits, and from time to time returned with legends of Tides and other unusual phenomena, were doubtless

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received with disapprobation and incredulity ; still more so, if they ventured to deduce the possible existence of a new continent, which as yet confessedly they had not reached, from evidences derived from drifting logs and a Sargasso Sea.

That is my view of the position ; and, unless we strangely limit the possibilities of progress before the human race in the æons of the future, surely the most advanced and modern man of science must admit, in a lucid interval, that posterity will regard him as one of the Ancients ; as one too, perhaps, who is pathetically struggling amid a welter of ignorance to hold fast to his traditions, to secure himself in his fertile little oasis of materialistic knowledge, to defend it against the hosts of barbarism, and to resist the unwelcome incursion of even friendly messengers from alien and distant lands.

Meanwhile, we are accused of lying, of megalomania, of folly, and of madness. Let it be so. I for one am in no hurry. I am not sorry that the present state of ignorance and prejudice surrounding this subject, in the minds of a large number of scientific men in the year 1912, should be put on record—lamentable though it be ; else posterity, familiar with a mass of developed knowledge, will hardly credit the curious obstruction which pioneers in this domain still have to encounter. Secure in the progress of the human race, we shall bide our time, cultivate our gardens, and pass on, before any wealth of fruits can be gathered in.

ON TELEPATHY AS A FACT OF EXPERIENCE: A Rejoinder to Sir Oliver Lodge

By Sir Ray Lankester, K.C.B.

I RECOGNISE with pleasure Sir Oliver Lodge's statement, that "we are not really unfriendly to each other," and endorse it. But I should be glad if this friendly feeling were to lead him to be more careful about attributing to me both words and opinions which are not mine.

He has done this and, I think, it is regrettable. He translates my ten hypotheses into terms of abuse, such as "liar" and "fool"—a form of expression which is offensive and has not been used by me. What is his object in thus inflaming the discussion?

He says that I take my stand on the assumption that every human faculty is already familiar and recognised by science. I have made no such assumption, either by specific word or by implication, and I do not hold any such opinion.

He says that I am "obsessed with the conviction of the impossibility of any discovery of a fundamentally new kind in psychology or psycho-biology." He has no justification for that statement. He cannot quote any words of mine by which it is warranted.

He implies that I argue from one experience with a trickster (Slade) as a basis for a comprehensive induction regarding "spiritualistic" pretensions. He must admit that this is not a correct statement, and that it is mere rhetoric. I base my induction on a very large examination and testing of cases.

It is a complete misrepresentation of my attitude to Sir Oliver Lodge and other believers in what is called the occult to say that it is determined by an *à priori* conviction of the "impossibility" of the reality of supposed occult phenomena. I have never taken

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that line. In my opinion the question to be answered is not "Is it *possible* that this supposed thing exists?" but purely and simply, "*Does* this supposed thing exist?"

I consider that it is not the business of an investigator of Nature to argue about "possibilities," but to seek for demonstration; and not only to be ready to examine supposed demonstrations of the existence of new and unsuspected things, but to exert himself in the attempt to test the supposed demonstrations of new things and to devise, if possible, demonstrations which are irrefutable. That, I believe, is admitted by all men of science as the rule of their activity.

Leaving all question of "possibility" aside, I say that Sir Oliver Lodge and his associates have not (in answer to the question, "Does telepathy exist?") given any demonstration of its existence nor even any evidence which makes its existence probable.

I observe that Sir Oliver Lodge claims the privilege of using the word "discovery" with an unusual signification. This is regrettable for his sake as well as that of his readers. When he said, some years ago, that the Society for Psychical Research had "discovered telepathy," he, no doubt, was able to reconcile the statement with his own conscience and regard for truth—but it was misleading to the public, as are his statements with regard to my attitude to these asserted "discoveries."

THE NEBULAR HYPOTHESIS AND ITS DEVELOPMENTS—I.

By Professor H. H. Turner, F.R.S.

It has been the custom of late years to speak disparagingly of the Nebular Hypothesis, emphasising the objections to it, and even asserting roundly that they are fatal. But this view is not taken by some thinkers who have notable claims on our esteem. In his *Leçons sur les Hypothèses Cosmogoniques*, which appeared not very long before his death, Henri Poincaré reviewed most of the alternatives which have been suggested at various times ; and summed up thus :—

“S’il n’y avait que le système solaire, je n’hésiterais pas à préférer la vieille hypothèse de LAPLACE ; il y a très peu de choses à faire pour le remettre à neuf. Mais la variété des systèmes stellaires nous oblige à élargir nos cadres, de sorte que l’hypothèse de LAPLACE, si elle ne doit pas être entièrement abandonnée devrait être modifiée de façon à n’être plus qu’une forme, adaptée spécialement au système solaire, d’une hypothèse plus générale qui conviendrait à l’Univers tout entier et qui nous expliquerait à la fois les destins divers des Étoiles, et comment chacune d’elles s’est fait sa place dans le grand tout.”

Henri Poincaré was accustomed to think in generalities. In presenting the Gold Medal of the Royal Astronomical Society to him in 1900, Sir George Darwin, himself an illustrious worker in the same field, said :—

“The leading characteristic of M. Poincaré’s work appears to me to be the immense wideness of the generalisations ; . . . to one accustomed rather to deal with the concrete . . . the easier process is the consideration of some simple concrete case, and the subsequent ascent to the more general aspect of the problem. I fancy that M. Poincaré’s mind must work in another groove than this, and that he finds it easier to consider first the wider issues, from whence to descend to the more special instances.”

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This illuminating remark from the English colleague (whose loss we had to mourn a few months after that of Poincaré himself) shows us that we need not allow the second part of Poincaré's judgment to discount the first. The nebular hypothesis was suggested by Laplace to explain the solar system ; and Poincaré gives it as his opinion that, for this particular case, it is satisfactory. He found himself unable to formulate, as customary with him, a much wider generalisation, of which the solar system provides us with a particular example ; but we need not therefore throw away the one asset. There may certainly be other methods of generating satellites from planets, or suns from suns ; even within the boundaries of our own system Sir George Darwin has shown us that the origin of the moon from the earth was of a totally different character from that of other satellites from their planets, or the planets from the Sun. The Moon probably separated from the Earth, as one drop of mercury divides into two, and reached its present distance by the action of tidal friction ; whereas other satellites were, according to the Nebular Hypothesis, abandoned by their primaries as rings, occupying nearly the position of their present orbits. There may be other modes of genesis—one such possible mode will form the subject of a future article—all of which may ultimately be brought under the generalisation which Poincaré failed to discern, and for which we may now have to wait long ; but meantime it is something to have his stamp of approval on the nebular hypothesis for the solar system ; and those unfamiliar with mathematical symbols may welcome a brief statement of his analysis in more general terms.

The nature of the hypothesis may first be briefly recapitulated. The solar system was at one time a diffuse nebula at least as large as the orbit of the outermost planet (Neptune, so far as we know at present), which had in some way acquired a rotation round an axis. During the subsequent history two things will remain unchanged, the total quantity of matter and the total quantity of rotation. The notion of the indestructibility of matter is to-day tolerably familiar, but that of indestructibility of rotation—in precise terms of “moment of momentum”—is perhaps less so, and a slight digression, which has an interest of its own, may be pardoned. There is a lecture experiment which vividly illustrates the point : a man stands on a turn-table—in these days of ball bearings

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it is easy to make one which turns without much friction—holding in his extended hands a pair of dumbbells, and a moderate rotation is communicated to him by a confederate. If now he drops his arms to his sides, his spin immediately becomes more rapid, for the reason that the measure of rotation depends partly on the mass (unaltered), partly on its distance from the axis of rotation, and partly on the rapidity of spin. Since the total quantity remains constant, what is lost in one factor must be made up in another. Dropping the arms to the sides diminishes the distance of the dumbbells from the axis of rotation, and this is made up by extra spin (in the third factor). The use to be made of the principle is almost exactly similar—as the rotating nebula is brought closer round its axis by the chilling effect of surrounding space, the spin increases. But, before returning to the nebula, let us justify the repetition of this well-worn illustration by remarking that it has recently been put to a novel purpose—that of demonstrating experimentally the rotation of the earth. Let the operator again take his stand on the turn-table with extended arms, but without asking his friend to rotate him : to all appearance he is at rest ; but we know that he is really being carried round by the Earth in its daily rotation. and therefore partakes of that rotation. (The easiest way of visualising this is to imagine him standing at the North Pole.) If then he drops his arms as before, his spin will increase, though not so markedly. To observe the increase it will be better to replace this rough apparatus by a much more delicate one, though the principle remains the same. When this substitution of a delicate mechanism is made, the increase of spin is manifested with complete success, and the rotation of the earth is thus conclusively demonstrated. And now, *where* do you think this experiment was made ? Why, at the Vatican ! where Galileo got into such trouble 300 years ago for asserting just this rotation of the earth which has in the last few years been so beautifully demonstrated by the Holy Father's own observer in his own observatory ! Surely the shade of Galileo must have presided at the ceremony !

We must return to our rotating nebula, which has meantime been cooling, and therefore contracting, and therefore increasing its spin. Assuming it to have previously attained a more or less compact form under the opposing action of gravity (tending to pull it together)

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and "centrifugal force" (tending to make it fly apart) the increase of spin will disturb the balance by helping the latter, so that a portion will become detached. It is an essential part of Laplace's hypothesis that the detached portion should first appear as a ring (like that of Saturn), which subsequently breaks up into one or more portions, which finally combine into a planet. But before solidification of the planet, the process starts again on a smaller scale, producing satellites from the planet just as it and its brothers were produced from the Sun.

This being the general nature of the argument we will now consider it in detail, using Poincaré's analysis. This analysis is partly his own and partly that of his great predecessor Roche : but we need not stop to consider in this article the respective claims of these and other workers ; nor those of Kant and Laplace to be the originators of the hypothesis ; let us rather concentrate attention on the conditions necessary for its success, in the light of this analysis and of modern additions to our knowledge of the solar system.

The first important condition, on which Poincaré lays great emphasis, is that from the outset the diffuse nebula must have a very dense central condensation ; in other words, the Sun himself must have been there from the first, the diffused nebula being merely his atmosphere, and not a diffusion of the now compact Sun. More precise meanings must be assigned to the loose terms "very dense" and "from the first" : by the latter is meant the time when it was first possible for the nebula to shed rings—that there is also a necessary antecedent history we shall see in a moment. By a "very dense" central condensation is to be understood one which absorbs the greater part of the total matter of the system : so that in calculating the gravitational attraction of the system we need only pay attention to this central nucleus and may neglect the attraction of the atmosphere upon itself.

Poincaré cites three separate arguments to show the vital necessity of this central condensation : one of them will suffice here. The quantity of rotation remains unaltered and was therefore the same "at first" as to-day : also the spin at the boundary is indicated by that which the outermost planet (Neptune) still retains ; it is thus very easily calculated that if we diffuse the mass of the Sun himself,

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to a distance from the axis, the product of the mass (known), spin (known), and the distance (assumed) will be too large for our available quantity of rotation.

We must start then with a central nucleus or Sun, surrounded by a diffuse and tenuous "atmosphere." Reserving for a moment the question of the physical nature of this atmosphere, we note that it must be rotating as a solid body rotates, without sliding of one part over another. Poincaré shows this clearly (using mathematical reasoning not easy to translate into every-day terms) by postulating the alternative and showing that it will not work. He arrives at this conclusion as a disappointment, for if the alternative had been admissible, it might have removed the formidable difficulty that a vast period of time is required before the atmosphere in question can arrive at such a state of steady rotation. The argument about the period of time is simpler, depending on a consideration of dimensions only, without following the history itself. We are familiar with a similar argument against increasing indefinitely the size of a bird: without knowing anything of the flying mechanism itself, we recognise that the strength tends to increase as the square of the bird's height or length, whereas the weight to be lifted increases as the cube, and will ultimately become too great for the available strength. By a similar argument Poincaré shows that the time for an atmosphere to reach the state of rotating like a solid increases as the square of the linear dimensions, which, starting from Helmholtz's data, brings us to billions or trillions of years before our original solar atmosphere can rotate as required. Is this a real or merely a sentimental difficulty? We have literally nothing to guide us to-day in estimating the length of time during which our system may have been preparing for the later stages of its history which we can dimly apprehend: the contemplation of such vast ages may give us a little catch at the heart, but their mere size in comparison with our brief lifetime is no good reason for rejecting them.

If we are ready to admit this condition of rotation, then it would appear that the physical nature of the "atmosphere" may vary within wide limits. Consider as one extreme our own atmosphere of gases. Near the Earth the vicissitudes of weather remind us that the air is by no means moving with the solemnity of a solid body: winds and storms run hither and thither: hot and cold

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currents reach us in turn, and though they are no doubt doing their best to lessen their differences, the hot currents parting with some of their heat to the cold as opportunity offers, these friendly interchanges avail but little so long as the unequally heated soil renews the causes of unrest. But we have learnt in recent years that the influence of the soil does not extend very high into the air ; about eight miles up this region of fickle weather ends, in favour of an "iso-thermal region" where the temperature remains the same for some distance upwards—how far, our "sounding balloons" have not yet told us. Possibly causes external to the earth induce still an outer shell of turmoil, but as yet we only know of these two, the inner shell of weather and the outer of climate. ("Weather," wrote the worried schoolboy, "is but for a little while, but climate is all the time.") So it may have been with the Sun's atmosphere. There may have been in the remote past the same turmoil near the central nucleus that we explore on the Sun's surface to-day with that wonderful modern instrument, the spectroheliograph ; but his diffuse atmosphere may nevertheless have reached in long ages the peaceful state of climate, of "all the time," of rotating as a united whole, without internal dissensions to speak of. And it may not have been made of gas like our air ; it may even have consisted of meteors—separate solid bodies—according to an investigation by Sir George Darwin in 1889, which has not hitherto attracted much attention (somewhat to the surprise of its distinguished author, who was by no means apt to over-estimate his own work).

"A meteor swarm is subject to gaseous viscosity which is greater the more widely diffused is the swarm. In consequence of this, a widely extended swarm, if in rotation, will revolve like a rigid body, without relative movement of its parts. Later in its history, the viscosity will, probably, not suffice to secure uniformity of rotation, and the central portion will revolve more rapidly than the outside (*Collected Works*, IV., p. 430).

The first sentence in this passage is welcome : the increase of viscosity with dimensions reduces the time necessary for attaining the state of rotation as a rigid body, above noted as a (sentimental ?) difficulty.

These essential conditions—the central nucleus and the rotating atmosphere—being postulated, Poincaré shows that the atmosphere

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will settle down into something like the shape of a mince-pie, with a critical junction between upper and under crust. We may imagine the mince-pie compressed, in which case some of the mince will ooze out at the junction ; but, though this pictures the right result, it is scarcely a true picture of the manner of attaining it. The pie is cooling, as in ordinary life ; but it is also spinning round its axis, we must remember, to which ordinary mince-pies are unaccustomed. The cooling increases the spin, so that the mince is flung out rather than squeezed out. But flung is too violent a word ; it rather dribbles out as the sand dribbles through the neck of an hour-glass. Poincaré's analysis shows that the increase of spin is equivalent to a slight opening of the edges of the pie, which allows of this dribbling ; and the escaped mince will form a ring like that of Saturn, thin and flat, and (what is very important for the further history of the movement) no longer rotating like a solid body as it did within the pie. In leaving the pie it has left also the region of climate and resumed that of weather, and this ultimately leads to its destruction as a ring. There are no perceptible signs of weather in Saturn's ring so long as we look at it in the ordinary way ; it seems to be rotating as smoothly and solidly as the planet itself. But Clerk Maxwell showed many years ago that this cannot be the case, or the ring would fall on to the planet ; and in recent years a beautiful experiment with the spectroscope has given us ocular demonstration that the inner parts of the ring are moving more quickly than the outer. It was Sir William Huggins who first showed us how to measure the velocities of moving objects by using Doppler's principle : if the object is approaching, we get more waves of light from it per second and the lines in its spectrum are shifted towards the blue end ; if it is receding, they are shifted towards the red end ; and the amount of shift is proportional to the amount of motion. Hence we can measure motions which are especially difficult to detect in other ways, just as it is especially difficult to see with our eyes the movement of a directly approaching distant train ; and by using this method, Maxwell's conclusions about Saturn's ring have been ocularly verified.

Let us consider what happens when an inner particle of the ring catches up an outer particle. If there is space between them, the inner will merely pass the outer without collision, as the earth

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continually passes an outer planet such as Mars. It is noteworthy, however, that at such a moment Mars will appear to us to be going backwards, just as a miserable cyclist appears to crawl backwards when we pass him in our motor car. This retrograding of the outer planet or particle will be seen in a moment to have important consequences. But now suppose the particles actually collide, owing to some accidental circumstance (perfect adjustment is the most unlikely of all), and are thenceforward entangled together. They will naturally go forward with a joint motion intermediate between that of the quicker and the slower. Because this motion is now slower than that suitable for the inner path, the double particle will tend to block the way for particles coming up on the inside ; and, because it is quicker than that suitable for the outer path, the double particle will crowd ahead on to its predecessors on the outside. For both reasons the first collision will tend to produce others, and they others in turn. In the part of the ring where collision and coalescence are started, they will develop rapidly, more and more particles being gathered together into one mass, so that a portion of the ring will be swept clear. We can imagine several centres of disturbance thus resulting in the break up of the ring into several distinct masses. Unless these are formed with a scarcely probable precision, their times of revolution will differ, so that one will catch up another, and what happened in the case of particles will happen for the aggregations ; the final result being a single aggregation containing all the particles which previously formed a ring. There is a beautiful nebula in the sky, the nebula in Andromeda, which, when photographed with modern instruments, shows a series of rings breaking up into patches in this way, and, if we may trust our eyes, even two satellites which have been formed from the break up of outer rings. The whole phenomenon must be on an inconceivably vast scale compared with Saturn ; but the scale may have little to do with the course of events in this instance. Since it was first photographed, by Dr. Isaac Roberts in 1888, this nebula has been generally regarded as a pictorial representation of the formation of a system, much as Laplace had imagined. Before 1888, the best drawings of the object had failed to indicate its real significance, chiefly from the difficulty of drawing accurately an object of which only a small portion can be viewed at one time.

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The rings will break up then, and ultimately a planet will be formed. But now we come to a fundamental difficulty. Whenever two particles collide and coalesce, the outer one is retrograding to the inner, and when they cling together this makes them waltz round each other as they go forward, with a combined motion closely represented in a modern ballroom : the dancers usually waltz in the same direction as the hands of a watch, but go round the room in the opposite direction. But the planets, as we know them to-day, are "reversing," so that there is an apparent direct contradiction between theory and fact, which can only be reconciled by supposing that the planets were originally waltzing and have now changed. The change may be made in one or other of two ways : firstly, by stopping the direct waltz, however momentarily, and then reversing, in the manner familiar in the ballroom. The second method is less suitable for trial on ceremonious occasions ; it consists in turning upside down while maintaining the same direction of spin. It is easier to convince ourselves of this alternative by using a watch, the hands of which will, we know, maintain their direction of spin even if we turn the watch upside-down ; but if we now visualise the hands through the back of the watch, they appear to be rotating in the "reverse" direction.

Now this twentieth century, young as it still is, has already furnished us with good evidence that in one or other of these ways the planets have changed their original waltzing for reversing. Some of them have left traces of their earlier life in their eldest children, who waltz round their parents to this day, in spite of the fact that the parents have meantime elected to reverse, and have instructed their younger children to do likewise. One or two doubtful instances of the kind were indeed known before, but they were ignored as unintelligible exceptions to the general rule ; new discoveries have brought the exceptions into line, and given them indeed an exceptional importance. To Mr. W. H. Pickering, of Harvard, belongs the credit of having opened up this most valuable line of investigation. He had no idea, when he entered upon a patient search for a new satellite of Saturn, of finding one with any marked peculiarity save that of faintness, and perhaps greater remoteness from Saturn ; nor did he when he actually found the ninth satellite in 1898 suspect the great value of his new treasure.

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Its special characteristic proved in the first instance a real embarrassment to him, causing him to lose track of the new satellite, and thereby exposing him to the jeers of hasty critics. The fact is that his new discovery, the eldest daughter of Saturn at present known, is revolving round him in the retrograde direction, as originally started by her father. Now it may seem strange that in calculating the orbit from the considerable series of photographs taken in 1898 on different dates, this cardinal feature should not have been apparent : but such is the fact, for a reason very familiar to astronomers. In the case of the orbit of a double star we always have two equally probable orbits to choose from, each of which might be the image of the other in a mirror. The twin stars may be waltzing in one orbit, or reversing in the other—we cannot say which without using the spectroscope, and that is difficult when the stars are faint. But in the case of a new satellite it was assumed without question that it was revolving round Saturn in the usual direction—it never occurred either to W. H. Pickering himself or to any one else even to try the other supposition. After losing his satellite for years, and searching for it for months he found it again in a quite unexpected place and then realised the exceptional nature of its revolution. But this must surely mean that the alternative orbits *can* be distinguished after all ? Yes, they can, and are, whenever the observer sensibly changes his position. In the case of a double star even, if we wait some millions of years, we shall see it from a sufficiently different point of view to enable us to choose the right alternative, even without using the spectroscope : for an object so close to us as Saturn, a single year suffices. Indeed, the difference between the two orbits is so marked that if the wrong one be used, the satellite will not be found near the computed place—as was Pickering's sad experience. Adopting the other alternative, all became smooth and clear, and in 1904, not only did he rehabilitate his lost discovery, but he added a totally unexpected type of satellite to our system. He had the gratification of pointing out its importance for the nebular hypothesis ; Saturn must originally have been waltzing when Phoebe was shed ; and his change to reversing must have occurred before the other satellites were born. Poincaré accepts this view, but he apparently prefers to regard the change as having been made by the ballroom method—stopping and going

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round the other way. The tidal action of the Sun is assigned as a sufficient cause.

There is, however, much to be said in favour of the other view—that without any stoppage of the spin, the axis of Saturn has turned topsy-turvy. First of all, Mr. Stratton has shown (in a paper which seems to have escaped M. Poincaré's vigilance) that the tidal action of satellites can bring about such an "inversion," and secondly the axes of the planets Neptune and Uranus occupy positions suggestive of being caught in the act of turning over. For the validity of the Nebular Hypothesis itself, it matters little which of these alternatives we adopt; in either case the new evidence favours the original waltzing of the planets, and goes far to demolish the difficulty raised by their present reversing.

Additional evidence in the same direction was soon forthcoming. The discovery of Phoebe stimulated others to look for corresponding outer satellites to Jupiter, and before the end of the same year (1904), Mr. C. D. Perrine had found one with the Crossley Reflector of the Lick Observatory. At this stage, the discovery of new satellites, regarded for a moment from the sporting point of view, was full of international interest. As will be seen from the following table, these discoveries had for many years all been made either by Englishmen or Americans, and the score was, England, 7; America, 6.

DISCOVERIES OF SATELLITES UP TO 1904.

	Mars.	Jupiter.	Saturn.	Uranus.	Neptune.	
Before 1685 .	—	4	5	—	—	Foreign.
1787-9 .	—	—	2	2	—	England.
1846-51 . {	—	—	1	—	—	America.
1877 . .	—	—	—	2	1	England.
1892 . .	2	—	—	—	—	America.
1899 . .	—	1	—	—	—	America.
1904 . .	—	—	1	—	—	America.
Totals .	2	6	9	4	1	

It was exciting to know who would get the next goal. The honour again fell to Mr. Perrine, who discovered a seventh satellite to

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Jupiter in February, 1905, making the score "seven-all." Visiting Oxford some months afterwards (on his return from the eclipse of 1905), Mr. Perrine presented us with a portrait of his new discovery—a tiny object so faint that he expressed doubts whether it could be photographed in our English climate. Almost at the time he was speaking it was being successfully photographed at the Royal Observatory, Greenwich, where they found it quite possible to follow the satellite regularly; and in February, 1908, Mr. Melotte of that Observatory had the satisfaction of sending England ahead again by the discovery of an eighth satellite to Jupiter, the most remarkable of all. Not only did it reproduce the peculiarity of retrograde motion, which the sixth and seventh had disappointingly failed to do, but it provided a case, new to the solar system, of a body which cannot be said to move in a closed orbit at all. We can represent the tracks of all the other known members of our system sufficiently closely by a circuit of wire; but for "J viii." we require a perfect tangle of wires interlacing one another like a conjurer's rings. The fact is that it is definitely trying to serve two masters—Jupiter and the Sun; and the vain endeavour causes confusion.

This new evidence leaves the case for the formation of planets (and satellites) in the manner of the Nebular Hypothesis very strong. There is one more point of general interest—the relation of successive planets (or satellites) to one another. What is the general nature of the recurrence of conditions for the birth of a planet? We should expect some regularity, even if it be only approximate. When a pot on the fire boils over, the fire is damped for a time, so that the water goes off the boil, then the fire recovers and makes the pot again boil over, and so on. The boiling over will only recur at precisely regular intervals if the amount of water which damps the fire is precisely the same every time, and if also it falls on coals which are of precisely the same shape and condition of glow, which is very unlikely, but short of precision there may be a rough tendency to regularity. The case of our rotating nebula, or nebulous atmosphere, is somewhat similar, but the boiling over is caused by cooling and not by heating. We have, however, to show the mechanism which causes the intermittence, and for this purpose we must again appeal to the distinction between the dense central nucleus and the rare atmosphere surrounding it. For simplicity let us suppose that

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the atmosphere has no mass at all ; then cooling it will indeed cause contraction, but there will be no resulting increase of spin, for having no mass the atmosphere does not count in the " quantity of rotation," which is concentrated entirely in the nucleus. But cooling the atmosphere will ultimately cool the nucleus : when it contracts, it will spin quicker, and this increase will be communicated, by friction or viscosity, to the atmosphere, even to the outermost layers, though this may take time. " Centrifugal force " will then overcome gravity, and a ring will be formed. The first " boiling over " will occur then when the chill which the atmosphere receives at its surface has had time to travel inwards to the nucleus, set it spinning more rapidly, and this increase of spin has had time to be transmitted back again to the surface. If the cooling stopped there, the boiling over would afford the necessary relief, and the atmosphere would settle down again to its steady rotation. But the boiling over has exposed a new surface to the cold of space, because the part that boils over will be drawn from the surface layers. Just as on our Earth's surface it is tolerably easy to move about horizontally but very difficult to jump up vertically, so at the surface of our mince-pie, or atmosphere, the surface layers can slip over those immediately below them very easily, and it is they which slip through the opening between upper and under crust when they get the chance. The formation of a ring practically takes the outer crust from the pie and gives the mince a new chance to cool. The surface cooling, as before, does not count towards increase of spin ; this new wave of cold must first reach the nucleus, and then a new wave of increase-of-spin is sent outwards, with the result that a second ring is formed ; and so may a third and fourth, etc., be formed. The process has a very fair resemblance to that of the boiling pot ; in each case the escape of matter, whether as water boiled over or as a ring abandoned, causes a chill, in one case to the fire, in the other ultimately to the nucleus. But there are essential points of difference. The chill to the fire is immediate and causes the boiling over to cease promptly, the interval to the next crisis being occupied mainly in allowing the fire to recover. The chill to the nucleus is transmitted slowly through the atmosphere, and its effect on the nucleus is slowly returned to the surface ; the interval between crises may be regarded as occupied chiefly in these two journeys.

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Consequently we may fairly expect it to be much more regular than its equivalent in our crude illustration of the fire. If the pot by some accident boils over with exceptional vigour, the fire will be considerably damped and will take a long time to recover ; if, on the other hand, a large ring is formed, the chill is considerable, but the time for it to travel may not be appreciably altered. A loud cry travels no quicker than a whisper.

Hence we may fairly look for considerable regularity in the deposition of rings by a Sun or planet by the method of the Nebular Hypothesis : and when we find it in the sequence of distances actually observed, we recognise a new piece of evidence in support of the hypothesis. Now this regularity was first pointed out more than a century ago by Titius of Wittenberg, though the name "Bode's Law" has become attached to it in consequence of the more prominent part played by Bode in drawing attention to it. The nature of it is exhibited below : a regular numerical series assigns successive distances for the planets approximately proportional to their observed distances. When the law was first enunciated, Uranus had not been discovered : when W. Herschel found Uranus in 1781, and its distance was found to fit in with the law, Bode urged that search should be made to find a planet for the gap in the series between Mars and Jupiter. The discovery of the first minor planet was curiously not a direct result of his organised search, but an independent and accidental find : but the gap has been amply filled in a manner wholly unforeseen at the outset—by the addition of a vast number of small planets (over 700 to-day, and no sign of an end to the list) which suit the missing term of Bode's Law in a rough average way. The discovery of Neptune in 1846 by mathematical analysis provided a new piece of evidence in regard to the law, which had so impressed the illustrious calculators that they both used it as a basis for their calculations, only to find ultimately that, though they had succeeded in deducing the place of the unknown planet with sufficient precision to secure its recognition in the heavens, its actual distance from the Sun was very different from that assumed by them. In his text book on *General Astronomy*, the late Professor C. A. Young writes :—

"In the case of Neptune, however, [Bode's] law breaks down utterly, and is not even approximately correct" (§ 488).

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Professor Young took merely the differences between actual and assigned distances, and Neptune is, on his plan, certainly a very conspicuous exception. It is, however, fairer to take percentages or ratios as below, when the anomaly is much reduced. Still the law is clearly only a rude approximation, and the same may be said of similar attempts to represent the distances of the satellites of Jupiter or Saturn.

COMPARISON WITH BODE'S LAW.

PLANETS.

	Real.	Bode's Law.	Quotient.	Difference from Mean.
Mercury	116	0 + 4	29·0	+ 0·3
Venus	217	3 + 4	31·0	+ 2·3
Earth	300	6 + 4	30·0	+ 1·3
Mars	457	12 + 4	28·6	— 0·1
Minor Planets	—	24 + 4	—	—
Jupiter	1,561	48 + 4	30·0	+ 1·3
Saturn	2,862	96 + 4	28·6	— 0·1
Uranus	5,755	192 + 4	29·4	+ 0·7
Neptune	9,017	384 + 4	23·3	— 5·4
Mean	—	—	28·7	—

SATURN'S SATELLITES (not given by Bode but on a similar plan).

	Real.	—	Quotient.	Difference.
Mimas	117	0 + 4	29·3	— 1·5
Enceladus	157	1 + 4	31·4	+ 0·6
Tethys	186	2 + 4	31·0	+ 0·2
Dione	238	4 + 4	29·8	— 1·0
Rhea	332	8 + 4	27·7	— 3·1
Titan	771	16 + 4	38·6	+ 7·8
Hyperion	934	32 + 4	25·9	— 4·9
Iapetus	2,225	64 + 4	32·7	+ 1·9
Phoebe	8,000	256 + 4	30·8	0·0
Mean	—	—	30·8	

Within the last few months an English lady, Miss M. A. Blagg, of Cheadle, Staffs, has pointed out that a much more regular and accurate law can be stated—one moreover which will cover the

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chief systems of satellites as well as the planets. She has not yet (February 28th) published the details, and indeed is still at work on some of them ; but the general nature of her result distinctly favours a greater regularity for the deposition of the planets and satellites than is indicated by Bode's Law. I hope to give some account of this discovery in the next number of this Review.

IMMUNITY AND NATURAL SELECTION

By G. Archdall Reid, M.B., F.R.S.E.

IN the last number of *BEDROCK* Dr. A. M. Gossage and Professor Elie Metchnikoff both imply a belief that degrees of human powers of resisting tuberculosis are wholly explainable by the supposition of acquired immunity. But while Dr. Gossage takes into account the admitted truth that animals and plants have arisen by evolution, Professor Metchnikoff does not; and while Dr. Gossage supposes apparently that the superior powers of resistance displayed by the individuals of some human races as compared to others is due to a vaccination (or some such process) occurring before birth, Professor Metchnikoff supposes it is due to a vaccination that occurs after birth.

It has been proved experimentally that, if a series of calves be inoculated with lymph taken from a man suffering from small-pox, they develop cow-pox. If subsequently the material be transferred back to a susceptible human being, the latter develops vaccinia. Cow-pox and vaccinia are, therefore, modifications of small-pox; that is, they are produced by "attenuated" forms of the same species of microbe. Small-pox is so virulent that it infects the whole body. The microbes reach the air passages whence they are exhaled into the atmosphere surrounding the sufferer. On that account small-pox is an *infectious* malady. On the other hand, vaccinia and cow-pox are merely *contagious*. Their microbes, limited to the point of entry, are not exhaled and therefore do not infect the atmosphere. I can conceive no probable reason why they do not spread over the body except that they are of such lessened virulence that they cannot, save when concentrated together, resist the phagocytes of the host. Detachments entering the blood stream

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are at once destroyed. When immunity is acquired the microbes become extinct, unless transferred by actual contact, or some analogous means, to fresh and susceptible individuals. It is significant that the members of a party of Esquimaux, a people that has lived for ages isolated from the rest of mankind and its diseases, who were taken to Berlin and vaccinated there, developed and perished of a general disease indistinguishable from small-pox. Their resisting powers were so low that even the microbes of vaccinia spread through their bodies.

What is the explanation of attenuation? What causes the decrease in virulence? Sometimes, doubtless, injury to the bacteria is the antecedent. Thus, if we expose the bacilli of anthrax to an abnormal but not immediately fatal degree of heat, they are attenuated. But injury does not explain all cases of decreased virulence. Generally speaking, the microbes of diseases are most virulent in that species of animal which is their normal prey, or in nearly allied species. Here they are in a niche to which evolution has fitted them. Transferred to a very distinct kind of animal they tend to perish, or, if they survive, gradually to lose their virulence for the first species and increase it for the second. In non-living media (*e.g.*, nutrient broth) also, the microbes, though they may flourish and multiply exceedingly, tend to lose their virulence. I do not know how all these changes can be accounted for except on the supposition that there occurs an evolution of virulence when there is need for it and a retrogression when there is no need, a hypothesis which is compatible only with the theory of Natural Selection.*

What is acquired immunity? Evidently it is a use-acquirement, a product of experience, a habituation.† It is analogous to the callosities we acquire in the palm of the hand as a protection against friction, to the power of resisting fatigue which results from continued exercise, and to the power of acquiring a high degree of immunity to the immediately poisonous effects (the intoxication) of opium which follows frequent indulgence.

How does vaccination or serum-treatment help immunity to develop? If a man swallow a dose of snake-venom (a toxin) a

* *The Laws of Heredity*, pp. 87—91.

† *Ibid.*, p. 246.

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thousandfold greater than that which is fatal if injected directly into his tissues, he is not only unharmed, but he acquires a greatly increased power of resisting injected doses. Presumably digested, altered, weakened, "attenuated" toxins enter his blood from his stomach, and habituation to these furnishes a stepping stone whence he is able to react to the stronger toxin and so acquire immunity. Many similar instances in which altered toxins help towards the acquirement of complete immunity are known.*

It is known also that bacteria, and presumably their toxins, tend to be injured, to degenerate, and to be destroyed in the tissues of infected persons. Digestion occurs here too; and there is present, therefore, in the blood of every individual, recovering from a microbic disease in which toxins occur, a scale of attenuated toxins up which he reacts till complete immunity is attained against the unaltered kind. Presumably antitoxic serum artificially supplies such a scale combined with digestive substances derived from the animal whence the serum is taken.† There can be no doubt about the habituation. It unquestionably arises in some diseases; and that is the important fact for us. Its occurrence implies, of course, some sort of physical change in the individual—some sort of growth, or development, or chemical, or molecular alteration.

Bacteriologists have published a score or a hundred contradictory hypotheses as to what precisely the change is. They are all mere guesses. Any number of alternative hypotheses may be conceived. We enter here a region in which there are as yet, as in metaphysics, no facts by which we may test our suppositions. We might as well speculate as to what is occurring on the other side of the moon.

If the bacilli of tetanus enter a wound they do not spread over the system, but, like those of vaccinia, are limited to the point of entry. Nevertheless the whole individual is poisoned. His distant nerve centres are affected and he is thrown into convulsions. If he dies, he perishes of general poisoning. If he recovers, he reacts in his whole body; and during recovery the bacilli are destroyed. His body has become an impossible habitation for them. Analogous facts are furnished by vaccinia, diphtheria, enteric fever, and many other diseases. *Acquired immunity is, then, in essence, an habituation*

* See *The Laws of Heredity*, chapter XII.

† *Ibid.*, p. 248.

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to toxins, not to the microbes themselves. In many diseases the virulence or abundance of the toxins may be judged roughly by the extent to which the individual is poisoned by them. In all acute diseases they are very poisonous or abundant. Hence the rapid illness. Hence also the swift death or recovery, a recovery for which Nature has provided by evolution—or by miracle. But immunity cannot be acquired with equal facility against all toxins. Every vegetable poison, for example nicotine, opium, digitalis, and strychnine, is, like bacterial poisons, a toxin, the function of which is to protect the organism producing it from its enemies. The human individual is able through continued experience to acquire enormously increased powers of resisting opium. Digitalis, on the other hand, is said to exercise a cumulative effect. So also the average individual is able rapidly and easily to acquire immunity (temporary or permanent) to common cold, chicken-pox, and measles; to the poison of rabies he is able to make no reaction unless helped by artificially attenuated toxins; while such diseases as leprosy and tuberculosis may linger in the system for any number of years.

Toxins in chronic microbic diseases are relatively feeble; or they are non-existent; or they are, like most animal and vegetable poisons, retained within the organism that produces them; for here we do not observe that rapid poisoning of the host that occurs, for instance, in tetanus.

The beginnings of chronic disease pass unnoticed. As Professor Metchnikoff says:—

“It is only rarely that the tubercle bacillus kills its victims in a short time, and, in fact, it requires at least several weeks to attain that result. In the immense majority of cases it burrows into the organism during months and years, with periods of intermission and recrudescences before arriving at the fatal result. There is, then, in regard to this matter, a very great difference between the bacillus of tubercle and that of plague, for instance, which never requires more than a few days—sometimes a few hours—to finish its victim.”

Additional evidence is supplied by the microscope. In acute diseases the phagocytes never engulf the microbes at the beginning of the illness. On the contrary, they are poisoned and kept at a distance by toxins which act at long range, but which are more

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concentrated in the immediate neighbourhood of the microbes. On the other hand, in chronic diseases, the microbes are engulfed from the first by the phagocytes. In tuberculosis, as stated by Professor Metchnikoff, they protect themselves by a waxen envelope. All the evidence, therefore, points to the conclusion that virulent toxins in any degree of abundance are not secreted into the tissues of the host. Hence the indefinite duration of the disease, *i.e.*, the failure to acquire immunity. The individual who is naturally very resistant does not fall ill even in the worst surroundings—not even in the slums of great cities where he constantly receives large doses of bacteria. If less resistant he may fall ill when in depressed health or in bad surroundings; in which case he tends to recover if improved in health or removed to better surroundings (*e.g.*, from slum to sanatorium). If still less resistant he contracts the disease, even under conditions in which most people of his race live unharmed, and then perishes hopelessly, in spite of every curative measure. In no case is there evidence of that peculiar reaction known as acquired immunity which enables the sufferer from acute disease, even when brought to death's door, even when swarming with microbes and drenched with their toxins, to make swift recovery—to ignore the toxins and destroy the microbes, and live afterwards inaccessible to the enemy.

It is true that tubercle bacilli render broth in which they may be grown very poisonous. Here we have evidence, not only of toxins, but also of their being set free in the medium surrounding the bacilli. But these very toxins, derived from broth, are used as tuberculin, a minute portion of which is sufficient to induce severe poisoning in a tuberculous person. Obviously, therefore, they cannot be abundant in his blood. Again, tuberculin in very minute doses is used as a curative agent, though, as Professor Metchnikoff states, it is of service in, at most, a very limited proportion of cases. But it cannot, of course, be curative if it is already present in abundance in the blood, in which case it would be as reasonable to attempt to sober a drunken man with another drink as to try to cure a person suffering from tuberculin poisoning with more tuberculin. It can be curative only on one or other of two suppositions. First, it may be an attenuated toxin. But its tremendously poisonous nature forbids this supposition, which, as far as I know, has

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been maintained by no one. In this connection it must be borne in mind that the tubercle bacillus, since it is normally earth-borne in its passage to fresh victims, is fitted by evolution for prolonged existence in the world exterior to the living body, and is, therefore, not easily attenuated. Or, second, tuberculin can be curative if, as I suppose in the case of the bacilli inhabiting the living body, it is retained within the microbes as an endotoxin, not excreted as an exotoxin—if it serves as a weapon of defence, not as a long range weapon of offence. I do not know if Professor Metchnikoff's waxy envelope plays any part in this relation; but, since infection does not cause those symptoms of poisoning which occur when tuberculin derived from broth or extracted by various means from the bodies of dead bacilli is injected, it seems clear that retention does occur. In which case the injection of tuberculin is useful because the poison then reaches all the cells, habituates them to itself, and so enables them to attack with advantage the bacilli when brought into immediate contact with them. Of course, I do not maintain that no toxin from the invading bacilli finds its way into the tissues of the host. The unlike behaviour of tuberculous and non-tuberculous persons when treated with tuberculin proves that some toxin, possibly from disintegrating bacilli, does escape. I suppose only that this quantity, as shown both from the behaviour of the individual phagocytes and of the whole individual, is not sufficient to cause poisoning nor to induce acquired immunity.

However, all speculation about tubercle bacilli and their toxins is more or less beside the mark. The facts do not supply material for certain conclusions. But it is beyond question that such diseases as tuberculosis and leprosy differ sharply from such maladies as measles and small-pox, which run a course that ends quite certainly within a definite time in death or recovery. In the latter we have no illnesses lasting, it may be, from extreme youth to extreme old age. Let a man convalescent from measles or small-pox and still weak from it plunge, under the worst possible conditions of environment, into the midst of infection, and he is sure not to re-contract the disease. But let a man convalescent from tuberculosis plunge under bad conditions into infection, and almost certainly he will fall ill again. Very plainly, in the one case we have "acquired immunity"; in the other we have it not. In the face of such

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facts it is merely an abuse of language to apply the expression even to complete recovery from tuberculosis. It would be as correct to describe a man who has recovered from a broken leg or a railway accident as having acquired immunity to broken leg or railway smash.

Dr. Gossage begins his essay with an exposition of scientific method in which he says he agrees with me, but in which I find he disagrees very considerably. I quote him in full :—

“ In the course of his argument Dr. Archdall Reid very properly demands the employment of the scientific method which consists, after the collection of observations, firstly in an inductive inference to explain those observations and secondly in a deductive appeal to reality, i.e. to an entirely different set of facts for confirmation. No theory can be regarded as established without the full carrying out of this method. In the appeal to reality it is, of course, necessary to make use of facts that are already, or can be, verified, and in the process of verification it is not permissible to employ the hypothesis that is to be tested as evidence of the truth of the facts, which must be established by quite independent considerations.

“ Following the exposition of the Scientific Method Dr. Reid claims that an appeal to reality proves the truth of what has been called the Selectionist theory of Evolution, the observations appealed to being the different resistances shown by different races when exposed to infection by bacterial diseases. For the acceptance of this ‘proof,’ one must first be certain that the observations relied on are really facts verified by independent evidence and reasoning apart from the theory itself. I do not mean here to imply that Dr. Reid is trying to establish his facts from his theory, instead of his theory from his facts, but the introduction of the arguments on which the theory was originally founded merely confuses the issue. After the narrowing down of the controversy to the appeal to reality I have therefore taken care to avoid these arguments. In the same way the destructive criticism of rival theories, which is an excellent and pertinent argument to use in advancing the theory, becomes out of place when seeking to establish the truth of the crucial test.

“ My contention has been that the evidence brought forward from human beings as the conclusive proof of the Selectionist theory fails in its object for two reasons. Firstly the facts relied on are doubtful and require verification, and secondly even if the facts are as supposed they would fail to afford evidence of evolution properly so called.”

Examine the last sentence of Dr. Gossage's second paragraph. Obviously he misunderstands. How is it possible to advance a theory (i.e., state it as an explanation) by means of destructive

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criticism of rival hypotheses ? And how can a supposition be tested except by means of such criticism ? The only kind of proof known to us is a demonstration that a given explanation accords perfectly with all the relevant facts that can be thought of and *that no other supposition can be conceived that does so*. The very name "crucial" indicates that a test is a guide-post situated at the parting of the ways ; for, in the case of every supposition at least two alternatives are conceivable—that it is true, and that it is not true (i.e., that some other explanation is true).

I think misunderstanding also accounts for the very vague sentence "I do not mean here to imply that Dr. Reid is trying to establish his facts from his theory, instead of his theory from his facts, but the introduction of arguments on which the theory was originally founded merely confuses the issue." I cannot imagine what Dr. Gossage means except that I try to prove my supposition by the facts on which I had based it—a particularly futile procedure. The following is the history of my attempt. I found that the theory of evolution through Natural Selection was disputed. Many other explanations of evolution had been formulated and were widely accepted. I wished to ascertain which of them was true. I turned to man, the only living being, not under artificial selection, that we can observe so minutely that we are able in nearly all instances to trace the exact causes of death and to ascertain the types of men that die. I found that many human beings die young and that they die mainly of microbic diseases which destroy the weak against themselves, leaving the strong to continue the race. It seemed to me manifest that some individuals were weak against tuberculosis, for example, and some strong. Here plainly was selection—a survival of the fittest, an elimination of the unfittest. If stringent selection occurs among the races of man, "Nature's rebel son" who has "escaped from selection," it is reasonable to suppose it occurs among wild plants and animals. Nobody, least of all Dr. Gossage, has attempted to impugn the correctness of my description of selection among human beings—for it is a mere description of the facts, not an inference from them.

But the fact that Natural Selection occurs is not a proof that it is *the* antecedent of evolution. It may have no effect in altering a race ; evolution may have quite other antecedents. Consequently

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I made appeals to reality. In effect, I said to myself, "If Natural Selection is the cause of evolution, then all stringently selective diseases and all other selective agents that I can think of and the effects of which I can trace clearly should be invariable antecedents of protective evolution. The whole of the facts should accord with this supposition, and they should fit no other explanation of evolution." Next I examined races to verify my predictions. In every instance in an endless series of instances I found *all* the facts accorded with the theory of Natural Selection and that *all* did not accord with any other supposition. This set of facts upset this contradictory supposition, that set of facts upset that contradictory supposition, and so on. As far as I am aware, in every case I based my hypothesis on facts drawn from the natural history of individuals and tested it by facts drawn from the natural history of races.

Now I want Dr. Gossage to substantiate his statement that I have used the same arguments (facts ?) both for the foundation and for the verification of my theory—or for "the confusion of the issues." I think he will agree that he ought not, under the circumstances, to be content with a general statement. It is apt to leave in his opponent a sense of unfairness. Will he, therefore, please name, at least, a single instance ?

The study of diseases is useful to the student of heredity and evolution, mainly because it enables him to see Natural Selection actually at work and to note with great precision the effects produced on hundreds of different races and sub-races of men and animals by a multitude of agencies which have acted on the different races with all degrees of severity for periods up to many thousands of years, and which have not only destroyed the weak against themselves, but have also in each generation changed the surviving individuals in all sorts of ways (as by injury, or by the acquirement of immunity) and which have even affected the germ-cells directly by altering their nutrition or bathing them in virulent toxins. Here is ideal material for the study of many problems of heredity and evolution. For example, we are furnished with decisive evidence that the Lamarckian doctrine and the hypothesis that variations are normally caused by the direct action of the environment on the germ-cells (*e.g.*, by the health of the parent) are false.* But, of

* See *The Laws of Heredity*, chapters IV., V.

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course, disease does not supply all the material available to the student of heredity and evolution. The major part of it has been gathered in other fields and some of the most valuable through experiment. In reaching conclusions we ought to take account of all the evidence. I have, at least, tried to do this in the book Dr. Gossage criticises. Only a section of it is devoted to disease.

I gather that Dr. Gossage supposes that the organic world has arisen by evolution, and that all explanations of evolution, except the Selectionist and Mendelo-Mutationist hypotheses, are, in view of the facts, inconceivable as true. The issue between us, therefore, is narrowed. He accepts, or at least is inclined to accept, the Mendelo-Mutationist supposition. He admits, apparently, that disease furnishes proof that selection occurs, that every race is resistant to every disease in proportion to its past experience of the disease, and that, if it be shown that the relative resisting powers of races are due to evolution, then the Selectionist hypothesis is proved and the Mendelo-Mutationist is disproved, or at least demonstrated to be improbable. But he declines to admit that these diverse racial powers are the product of evolution. On the contrary, he formulates hypotheses that they may be due to individual acquirement, or to that transient kind of evolution which, as he supposes, is quite different from the permanent kind that is founded on mutations. In fact, he suspends judgment. If my proofs are insufficient, he is right, of course. If they are sufficient, I know of no more unscientific attitude. That way lies futility.

I will deal with his arguments from disease presently. Meanwhile consider his general attitude. His sole alternative to evolution by selection is evolution by mutation. If it can be shown that the facts (whether used as foundations of hypotheses or as tests of them) accord to the minutest detail with the theory of selection, while many of them do not accord with the Mendelo-Mutationist supposition, then, logically, he has no choice but to accept the former and reject the latter. No mutations are known to occur in relation to microbic disease. Therefore he bases his inclination for the Mendelo-Mutationist hypothesis on evidence drawn from other sources. But, when, on that very evidence it is claimed that every item of the whole series of Mendelo-Mutationist suppositions is demonstrably an erroneous guess, he replies, in effect, "I do not

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want to be bothered with all that. At present I wish to limit attention to disease. Prove to me that disease causes evolution by the selection of fluctuations. If you cannot I shall consider myself free to hold the alternative explanation." To illustrate his method—Harry has been murdered. It is agreed that Tom *or* Dick slew him,—that every other explanation is inconceivable as true. The advocates of the one supposition bring evidence that Dick did not, and could not possibly have murdered Harry. Their opponents reply, "Don't bother us with all that. Prove by facts to which we limit you that Tom did it."

His attitude is very characteristic of the followers of the "new science." In the beginning the Mendelo-Mutationist hypothesis was heralded by an enormous flourish of trumpets, "We are the true exponents of science. Our results are experimental, and therefore comparable to those of the physicists and chemists. We labour in the breeding-pen. Other people are armchair philosophers and essayists." And so forth, and so forth. But presently men began to question and say, "When the facts are given, the receiver is placed in the same position as the observer, and it is as easy to think in an armchair as in the breeding-pen. The Mendelo-Mutationist facts are certainly true and very remarkable and valuable. Their discoverers deserve high honour. But observation is one thing, interpretation quite another. Are the only facts known to us about living beings derived from experiment? If one twists a dog's tail experimentally and elicits a howl, how is the howl a more certain or intrinsically valuable fact than the tail? Why should the howl be accepted as something glorious, and the tail ignored? It is true that physics and chemistry are accurate and experimental. But is it not true also that they are accurate because it has been possible to measure and test minutely, and experimental because the facts could be gathered in no other way? Mathematics, the most accurate of all sciences, is not experimental; nor is astronomy, another very accurate science. The offspring and descendants of grey rabbits crossed with white rabbits are unblended grey *or* white; the offspring and descendants of white and black human beings are mulattoes in whom almost every character is blended perfectly and permanently; in explaining, why should the former set of facts be accepted and the latter ignored? Does, indeed, the claim as to the

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exclusive value of experiments mean anything more than an appeal to ignore much indisputable evidence and to found guesses on evidence so fragmentary that they cannot be tested ? Why are guesses founded on experiment, which is a mode of observing, not of thinking, more entitled to respect than other guesses ? Can an exceedingly complex matter, involving a vast multitude of facts and issues, be adequately dealt with except in 'essays'—i.e., by a careful comparison of *all* the facts and issues ? How can experiments on, at most, half-a-dozen generations justify the statement that mutations persist for ever unless eliminated by selection ? Is there not evidence that *all* characters tend to retrogress on cessation of selection ? How can no experiments at all prove that continued selection does not result in evolution as permanent as any known to us ? It is claimed that the alternative reproduction of characters which so commonly occurs when domesticated varieties are crossed is proof of their discontinuous origin. But natural varieties tend to blend when crossed ; is, then, this not proof that they came into being through the selection of fluctuations ? If there is segregation of allelomorphs, how does it happen that purely bred artificial varieties have, in thousands of instances, reproduced the very traits which they are supposed to have lost utterly ? How does it happen that natural varieties, even when crossed, never or very rarely reproduce latent traits ? Is not this proof that Nature selects fluctuations, while man (as is admitted) selects mutations ? Are not, then, Mendelomutationists victims of a gigantic practical joke played unconsciously by the human breeder ? ” and so forth, and so forth.

Immediately the leaders of the “ new science ” went into retirement. To none of these inquiries did they reply ; but to their critics they intimated, “ The time for labour is now ; the time for controversy not yet.” To their followers they said, “ Continue to have faith. We are labouring in the breeding-pen. We are employing holy experiment. Presently our enemies will be scattered.” So the matter stands—unique in science, common enough in religion. It seems that nothing now can tempt Mendelomutationists from their holes. You may use blandishments, and you may use insults. You may tweak their noses and they will turn their tails. You may twist their tails and they will only snuggle deeper. At most, you will elicit a miserable squeak that

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you, who perhaps have not so much as crossed a pair of mosquitoes, are a contemptible, unscientific fellow. Contrast Huxley in his wrath smiting misbelievers into silence. Contrast Newton and Darwin, patiently considering and answering every objection—every alternative supposition. Contrast any man who is sure that he is right, and thinks that he can prove it. Nay, contrast any man who is not a partisan, but simply a truth-seeker.

My expressions are brusque ; but, as almost always happens when considerations other than pure reason dictate belief (or a demand for belief), the whole discussion has been characterised by extreme brusqueness. Compare the calm discussions of mathematicians, physicists, and chemists with the savage disputes of religious sectarians, Mendelians, and biometricians. Here, for example, is Professor Bateson's opinion of Professor Pearson and all his works :—

“ Of the so-called investigations of heredity promoted by Professor Pearson and the English biometrical school it is now scarcely necessary to speak. That such work may ultimately contribute to the development of statistical theory cannot be doubted, but as applied to the problem of heredity the effort has resulted only in the concealment of that which it was ostensibly undertaken to reveal . . . To those who hereafter may study this episode in the history of biological science it will appear almost inexplicable that work so unsound in construction should have been respectfully received by the scientific world.”

Professor Pearson has replied in suitable terms. Just as the experimental school insists on the exclusive value of experimental observation, so the statistical school (or some leaders of it) insist on the worthlessness of all but biometric observations. It is as if two observers, the one using a telescope and the other a microscope, had denounced not only each other, but also the man who, while accepting their facts and trying to square his hypothesis with them, had employed, as occasion served, his naked eye as well.

If it be thought I exaggerate, the reader has only to turn, almost to any essay by Professor Pearson or the leaders of the experimental school. He will find the usual beginning, and often the ending, is a severe, minatory declaration that the time for vagueness and inaccuracy has passed, and the time for scientific exactness come. Something will be said also about modern science and modern

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methods. Thus the reader's mind is prepared. Next will follow a detailed account of observations which, in the case of the Mendelians, are often of the highest value. Lastly, there will be a series of wild guesses, the correctness of which is assumed, not because it has been shown that all other interpretations of the very facts used are inconceivable as true, but merely because those facts were gathered in a certain way. The whole proceeding is, in fact, of the nature of a confidence trick, with the remarkable feature that professors and dupes are alike persuaded of, but alike unable to defend, its honesty.* If the reader is still unconvinced I can only ask him to note that no one will reply to me. I venture to believe that effective reply is impossible. For it is beyond question (1) that all relevant and verifiable facts, no matter how gathered, are equal before science ; (2) that every hypothesis is a mere guess until it has been demonstrated that it accords perfectly with all the facts *and that no other explanation does so* ; (3) that experiment and statistics are especially exact not on all, but only on particular occasions ; (4) that the vast mass of our precise information about living beings, as about most other things, is derived from simple observation ; (5) that therefore the exponents of the "exact" methods have not so much used as neglected precise evidence ; and (6) that, owing to the scanty and

* Biology has been extraordinarily fertile in unending controversies, but quite the most futile of all has been the Mendelo-biometric. Each side collected facts, which, in the main, were quite accurate and bore obviously on the points at issue ; each formulated hypotheses which, in view of the facts on which they were based, were conceivable as true ; but thereafter each broke every established rule of the game. Setting up private standards as to what constitutes evidence and what proof, they decided that one another's facts were worthless (*i.e.*, untrue or irrelevant), and on no occasions appealed to crucial instances. Indeed, their facts, collected in only a single way (as it were, from only one witness) and drawn from no more than three or four generations of individuals did not furnish means for testing. They conducted themselves exactly as contending Mahomedans and Christians who in their disputes use respectively only Mahomedan or Christian data ; or like two players of a game who employ different instruments and abide by different rules. They had, therefore, no common platform ; they did not begin by agreeing to take into consideration all verifiable evidence, and to test every hypothesis by tracing its consequences and then turning to Nature to see if those consequences really followed. Ultimately they agreed—the only point of agreement possible to them—that argument with opponents so unfair and unreasonable was folly. Here we had a contest between the whale and the elephant as to which had discovered the art of flying.

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fragmentary nature of their materials, every one of their suppositions is demonstrably a guess, and in most cases an erroneous guess. It will be easy to prove me wrong by instancing at least one supposition which is not merely a guess, but no one will attempt it.

Consider, now, Dr. Gossage's arguments from disease. Malaria varies in severity in different places and seasons. It is more virulent in parts of West Africa and in the Terai than in Europe. It is less severe in winter and spring than in summer and autumn. Doubtless, sufferers are infected by larger doses in some places and seasons than in others. Skilled observers declare that they are able to distinguish the kinds of parasites that produce the various types of malaria—intermittent, remittent, quartan, tertian, æstivo-autumnal, and so on. Some Italian observers contend that the different types of malaria are produced by distinct varieties of parasites, not interchangeable, though closely allied biologically. Laveran, on the other hand, contends for the unity of the forms, which he regards as modifications of one polymorphic parasite. I argued from the fact that, since in the same time and place European troops suffer more severely than their native comrades, the former were less resistant than the latter. Dr. Gossage now argues, apparently, that the different varieties of malaria parasites have a predilection for different races of men. But surely, unless we suppose that the microbes are influenced by political animus, this implies a difference in resisting power between the human races—which is precisely what I maintain.

Dr. Gossage's principal contention is that races that are familiar with a disease are more resistant to it than races that are less familiar merely because there is individual acquirement—merely because the individuals of the former races have been inoculated through parental disease and so have acquired some degree of immunity. *I want now to emphasise as strongly as possible one fact which appears to me decisive, but on which, hitherto, I have been unable to persuade him to fix his attention.* On the average, individuals are resistant to the different diseases not so much in proportion to the *sufferings* of their *parents*, but in proportion to the *deaths* occurring in many generations of *ancestors*. Thus the negro race in America has been much exposed to tuberculosis for about four hundred years; the white races dwelling with them for immensely

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longer periods. During those four centuries, the negroes have suffered, and do now suffer, to a far greater extent than the whites. On Dr. Gossage's hypothesis, they should be much more thoroughly inoculated, and, therefore, much more resistant than the whites. How does it happen, then, that they still remain, relatively speaking, so very susceptible that it is said, "Every other adult negro dies of consumption." That which is true of negroes and whites in relation to tuberculosis is true of all races in relation to all lethal diseases. Thus negroes from Barbadoes, who have not suffered from malaria, but whose ancestors suffered much, contract the disease when taken to West Africa, but recover far more easily than their white officers who have not previously experienced it. Will Dr. Gossage explain how this fact is compatible with his supposition ?

We know that races (*e.g.*, the negro and the Anglo-Saxon) differ sharply in their obvious physical characters. I supposed, on what seems to me abundant evidence, that they also differ as sharply and innately in their powers of resisting the various diseases. Dr. Gossage contends, in effect, that all races are innately alike with respect to disease and differ only by acquirement. The reader must choose between these two suppositions. If he, as I think he must, accepts the supposition that the differences are innate, then the next task is to account for them. The fact that every race is resistant to every disease *in proportion* to the *length* and severity of its past experience of it furnishes a guide which, as it seems to me, indicates the truth very plainly.

But Dr. Gossage contends that even if it be taken as proved that selection by disease causes evolution, yet there is no evidence that this evolution is of a permanent sort. In vain I have asked how he knows that there is, or ever has been, any permanent evolution of the kind he means—evolution which persists when selection ceases. As far as I am aware, his belief has nothing to rest on except a very shocking guess made by Professor Punnett and others to the effect that mutations, unlike fluctuations, persist eternally unless eliminated by selection—a guess made without any foundations of fact or testing by facts, and in spite of a vast array of facts. Consider how parts that have become useless continue to retrogress long after they have become vestigial and lost all selection value of any sort. Combine with this the thought that mutations, if permanent, could

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not, owing to presence of "impure dominants," be eliminated except by exceedingly stringent selection. Consider how vastly embryos, closely protected *in utero*, differ from their numerous, very divergent, remote ancestral prototypes. They have lost immensely more than they have gained. Consider the rarity of mutations and the fact that they are nearly always not only useless but positively injurious. Consider the extreme adaptive ductility of species in changing environments. Then judge the value of the guess. It is remarkable that while Professor Punnett and others account for evolution by the selection of mutations, Professor Bateson and others repudiate selection altogether. "We look on the manner and causation of adapted differentiation as still wholly mysterious."* In other words, Professor Bateson accepts evolution (*i.e.*, adaptation) as the explanation of the existence of animals and plants, and supposes it is founded on mutations; but perceiving, apparently, that the selection of mutations cannot lead to adaptation, he rejects Natural Selection as the explanation of evolution, and has no suggestion of his own to offer.

It is unnecessary to deal at length with Professor Metchnikoff's article. He shows that tubercle bacilli defend themselves not as tetanus bacilli do, by keeping the phagocytes at a distance, but by the secretion of wax envelopes. He insists on the long duration of tuberculosis and on the doubtful success of all treatment—climatic, medicinal, vaccinal, serum, and the like. [He observes that scrofulous people tend to be immune from pulmonary disease. He notes that savages are more susceptible than civilised peoples. He assumes, apparently, that all individuals of all human races are equally susceptible, but that some survive because their first infection is by mild strains of bacilli while others perish because from the beginning they are infected by more virulent types. In brief, he supposes that bacilli differ innately among themselves; but he does not give the smallest hint that he has considered the possibility of human differences. A certain number of facts are gathered on which to found a supposition; all the rest which do not accord with that supposition are completely ignored. It is by this process precisely that interpretative biology and bacteriology have been made hotbeds of faction and tumbling grounds for whimsies.

* *Darwin and Modern Science*, p. 99.

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It is a known fact that the organs of the body differ in their susceptibility to various diseases. The microbes cannot get to some organs, or the conditions in them are unsuitable. Thus ringworm and typhoid do not affect the lungs or liver, and tuberculosis of the muscles is, at least, uncommon. Of all the organs of the body, the lungs are most exposed to infection by tuberculosis, and, seemingly, the least resistant to it. Nowhere else does the disease spread so rapidly, and, as Professor Metchnikoff indicates, nearly all infection, save perhaps of the skin and alimentary tract, is by the air passages. But it does not follow that a person who is resistant in the lungs is correspondingly resistant in other organs. If infection is by way of the lungs, and if the lungs escape disease and the glands are infected, here is evidence that the former are resistant and the latter relatively more susceptible. But Professor Metchnikoff supposes, in effect, that virulent bacilli do not (and therefore, cannot) pass beyond the lungs, but stay there and flourish; and that less virulent bacilli, which are unable to persist in the lungs, pass through them and take up a prosperous residence in the glands. In other words, he credits the virulent bacilli with a penchant for the lungs, and the less virulent bacilli with a penchant for other organs. Moreover he supposes that bacilli, which have been unable to establish themselves in the lungs and have settled elsewhere, confer an immunity which prevents settlement in the lungs of subsequent invaders. But in that case, how is it that immunity is not conferred on tissues similar to those diseased? When one gland is diseased others tend, one after another, and it may be after long intervals of time, to become diseased also. When the skin is diseased, why is immunity not conferred on it? Not only does it continue diseased, but the malady tends to spread.

Bacilli owe their survival to their virulence. If the theory of evolution is true, they are derived from saprophytic ancestors which became fitted, by means of their increasing virulence, to invade living bodies and to maintain themselves there. I take it that no explanation of this evolution is conceivable as true save Natural Selection.* The less virulent strains tend to be eliminated, the more virulent tend to survive. Professor Metchnikoff supposes that

* *The Laws of Heredity*, p. 89.

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where tuberculosis is prevalent human beings owe their survival to a preliminary vaccination by the weaker strain. But, once again, are tubercle bacilli influenced by political animus and race hatred ? How does it happen that, while civilised peoples (*i.e.*, peoples who have had a long ancestral experience of tuberculosis) tend to become vaccinated, savages (*i.e.*, people who have had little or no ancestral experience of the disease) that have entered the civilised environment or had it brought to them, tend to perish ? And not only savages, but their descendants for many generations. If the hypothesis of race hatred be not maintained, what is it that causes one type of bacilli to make the initial attack on the civilised peoples and another type to make the initial attack on the savages ?

Is it not plain that every hypothesis, which is contradictory to Natural Selection, is wrecked by this fact, that *races are resistant to disease in proportion to their past ancestral experience of it ?*

THE SUPPRESSION OF VENEREAL DISEASES: An Australian Experiment

By James W. Barrett, C.M.G., M.D., M.S.,
F.R.C.S. (Eng.)

INTRODUCTORY NOTE BY PROFESSOR E. H. STARLING,
M.D., F.R.S.

Syphilis is widespread among our population and rivals tuberculosis as a cause of incapacity, sickness and misery. It is responsible for a large amount of nervous disease and insanity, and is an important factor in infantile mortality. Though a prominent part in its spread is played by immoral intercourse, more than half of the sufferers are innocent and unwitting. It acts as no deterrent to immorality, nor is the punishment proportional to the offence or limited to the offender. Yet of all chronic infectious disorders of man, it should be the easiest to stamp out. It is only transmitted by heredity and by direct personal contagion, and is not conveyed by the lower animals or by insects. Modern discoveries by Wassermann and by Ehrlich have added enormously to our powers of diagnosing the disease (even in its latent stages) and of curing it. It is amazing that a civilised community such as ours should have made no concerted effort to abolish this plague from our midst, a result which might probably be achieved in twelve months, if a free hand could be given to medical science.

Now that the responsibility for the national health has been assumed by the State, it is important that we should know what can be done in this direction by a community with the same ideals, prejudices, and

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hypocrisies as ourselves, and at my request Dr. Barrett has kindly, in the following article, given a brief account of the measures which have been already taken in Melbourne for the diminution and ultimate suppression of venereal disease. Is there any reason why similar measures should not be adopted in this country ?

E. H. STARLING.

DURING the last few years a remarkable piece of work has been done in the State of Victoria, Australia, by the Government of that State, through the Ministers of Health, Mr. Hazelthorpe and Mr. Edgar. The actual details were arranged by the Chairman of the Board of Health, Dr. Barnett Ham, who had at his disposal a representative committee of medical practitioners, including men and women. The result has attracted widespread professional notice, and was placed before the Royal Society of Medicine, London, during the current year. As the problem, however, can only be solved by the statesman and physician in combination, public education has formed an essential part of the Australian programme. It has been therefore suggested that the facts should now be placed before the public of Great Britain.

For some years a controversy existed in Australia respecting the extent of distribution of venereal diseases. It centred largely in the State of Victoria. On the one hand the pathologists, the oculists, and the specialists in diseases of children asserted that syphilis was responsible for a vast amount of damage to mankind. The injury was not only direct, but in addition syphilis, by lowering bodily resistance, was indirectly responsible for other infections, for example tuberculosis, and other states of malnutrition. On the other hand, some of the surgeons and many physicians took the opposite view. They said that the grosser manifestations of syphilis were diminishing in frequency and that the supposition that indirect troubles were so caused was not based on fact.

What was the evidence ? Two sets of 100 *post-mortem* examinations were conducted at the Melbourne Hospital on the bodies of persons who had died from many varied causes. The selection was indiscriminate. In each set about 30 per cent. of the subjects showed appearances in arteries and organs which the pathologist

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to the hospital interpreted as syphilitic in origin. Syphilis was not the direct cause of death in most of these cases, but was regarded in some as the indirect cause, *i.e.*, the subjects, if they had been free from syphilis would not have died from the injury or disease for which they were admitted into the hospital.

Oculists, who have the opportunity of examining in eyes changes necessarily invisible in other parts of the body, found that certain appearances were associated in some cases with a history and with other signs of syphilis. They then assumed that these appearances always meant syphilis even when the other evidence was wanting. Children's specialists have similar opportunities of judging, and for the most part took the same view.

It should be remembered that, for various ethical and other reasons, patients frequently deny the acquisition of syphilis, and in some cases undoubtedly acquire it without being aware of the fact. Furthermore, a certain limited number of cases do occur in which the infection is accidental and extragenital. Yet we know from careful insurance medical records that the great majority of men put themselves in the way of infection before marriage.

To this statement of the case it was replied that the facts were indisputable as far as they went, but that many of the inferences were unwarranted. In particular doubt was cast on the arguments based on the *post-mortem* and ophthalmic appearances.

This then was the position in 1908, when the Australasian Medical Congress met in session in Melbourne. After prolonged discussion the following resolution was carried : " That syphilis is responsible for an enormous amount of damage to mankind, and that preventive and remedial measures directed against it are worthy of the utmost consideration." This resolution was at once conveyed to the Government, public interest was aroused, and a deputation of the clergy waited on the Premier, Mr. Murray, presented their view of the matter, and asked that action be taken. The Government then consulted their officer, Dr. Ham, who advised them that a comprehensive enquiry should be made into the matter by utilising the then novel test for syphilis known as the Wassermann blood test. On receiving the necessary authorisation and financial allocation, the Minister of Health gazetted his advisory committee of medical practitioners to act with Dr. Ham, and the work began.

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The first problem to be settled was the controversial matter above alluded to. By the Wassermann test it was now possible to say with accuracy who was and who was not suffering from syphilis, and consequently to place matters on the secure foundation of fact. For twelve months syphilis was made a compulsorily notifiable disease in an area of ten miles radius from the General Post Office, Melbourne. No names were given, but the age, sex, and clinical conditions were to be accompanied by a specimen of blood, which was submitted to the Wassermann test. A special staff was employed for conducting the work. A small fee was paid to any practitioner who so desired for the tedious task of reporting and blood collection. Practitioners were asked to report not only obvious cases of syphilis, but also cases which might be thought to be indirectly so caused. The profession on the whole responded well, although probably only a small proportion of the available cases were caught in the net. At the end of the year about 5,500 cases had been reported and tested. Of 5,000 of these, 3,167 were proved to be syphilitic, i.e., .5 per cent. of the population. It does not, of course, follow that this percentage really represents the amount of syphilis in the community. It does follow that at least .5 per cent. are definitely known to be infected. Of the 5,000 cases reported 900 came from one hospital. Many practitioners did not report any cases.

At the same time two other lines of investigation had been followed at the Victorian Eye and Ear Hospital. Five hundred and fifty cases had been examined in four months on the following plan. During that period every new patient visiting the hospital on Monday and Thursday afternoons (eye cases) and on Tuesday and Friday afternoons (ear and throat cases) was submitted to the Wassermann test, irrespective of the disease or condition which had caused them to visit the hospital. Many came to be fitted with glasses, to have specks of dust removed from the eyes, etc. Of those who attended on Mondays and Thursdays 13 per cent. were found to be syphilitic, and of those who attended on Tuesdays and Fridays 15.8 per cent. were syphilitic, or, if the whole 550 were considered, 13.3 per cent.

This, so far as can be ascertained, is the only attempt yet made to determine the incidence of syphilis in a considerable fraction of a population, taken at random. At the same time at the Children's Hospital an enquiry was being conducted respecting the value of

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the disputed *post-mortem* appearances. A pathologist examined the tissues of the bodies of those who showed signs of syphilis, whilst the blood was independently examined. A comparison of the accrued results confirmed the position originally taken up.

The investigation then showed that the extent of distribution of syphilis is great, that the evidences of its existence were valid, and that the damage done must be enormous. Before, however, any further action was taken, another phase of the problem loomed up. Syphilis is communicable by genital and extragenital means, and is transmissible to children. The other and commoner form of venereal disease, gonorrhœa, is transmissible by genital means only. At this stage the staff of the Women's Hospital, Melbourne, informed the Government that in their opinion half of their operative work was due to gonorrhœa and that much sterility was due to that cause, and that if an attempt was to be made to suppress one venereal disease it would be better in the public interest to deal with all such diseases.

The report of Dr. Ham and the committee, together with the evidence on which it was based, was now presented to the Government and handed by the Government to the daily Press for publication. The gravity of the position was realised by the public and constructive action became imperative. The Government appointed a permanent advisory committee of medical men and women, under the presidency of Dr. Ham, to report to and advise it through the Minister of Public Health. This committee was faced with the specific knowledge that venereal diseases were responsible in Victoria for a vast amount of suffering and misery. Whether the hypothesis held by some practitioners—that syphilis is the principal cause of nearly all disease and death prior to senility—is correct or not had not been proved. Adequate evidence, however, had been furnished to indicate the magnitude of the problem. The first steps taken were educative. A right understanding of the facts was a necessary foundation for public action.

The management of these diseases has always been complicated by the admixture of practical medicine with morality. Medical men dislike vice perhaps rather more than most people because they see so much of its nauseous side. But medical men as such are not professors of morality ; their primary duty is to prevent and cure

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disease. Now the popular theory that the sufferers from venereal diseases are rightly served accords very little with the facts. The crafty and the cold-blooded can generally avoid the consequences of their acts, while the impulsive and simple-minded are more liable to suffer. But apart from this consideration, there remains the fundamental fact that probably at least half of the sufferers from gonorrhœa and more than half of the sufferers from syphilis are not in any way responsible for its acquisition. These diseases are conveyed in marriage by husbands to wives or *vice versâ*, and in the case of syphilis from parents to children. Consequently more than half the sufferers are clouded with reproach unjustly, and seek treatment by secret and often unsuitable methods.

The first business of the committee was then to intimate publicly that, whilst members would informally support any moral campaign, as a committee morality was not their business. Their object was to teach people to prevent infection and render those infected no longer dangerous to other people, and to do this by open educational means. The world is not rendered more or less moral by permitting people to become infected with syphilis or gonorrhœa. To effect these ends the aid of influential ladies was sought and the National Council of Women was informally approached. The Council convened a meeting of its members, listened to sober statements of fact from medical women, and passed a resolution offering its co-operation with the Government in any reasonable steps which might be taken to diminish suffering. The proceedings were temperate and dignified. At the same time it informally indicated that the Council would oppose any institution of a Contagious Diseases Act unless it applied equally to men and women. This, of course, would render any Contagious Diseases Act impossible, but the Governmental committee was in any event not at all in favour of such an Act on purely medical grounds. They thought that the problem would be better attacked in another manner.

The Press was next interviewed and urged to publish official and unexpurgated accounts of the steps taken, to call these diseases by their proper names, and in general to abandon the usual attitude of (civilised) make-believe. This request was agreed to, and such reports have been systematically published.

The Government then decided through the then Treasurer,

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Mr. Matt, to furnish and equip a ward in the Alfred Hospital and the Women's Hospital, Melbourne, for the treatment of people of any class, except prostitutes, who were suffering from these diseases in contagious forms. The object was not so much solicitude for the sufferers as a desire to prevent further infection. The profession was circularised and asked to send such cases for indoor treatment. Whilst under treatment sensible advice can be given. These wards are specially staffed and the Government defrays the cost of maintenance. Prostitutes are otherwise provided for by the principal medical officer and by their own medical attendants, who have been urged to co-operate in this campaign.

The Government has further arranged with the University of Melbourne for the free application of the Wassermann test to 2,000 hospital cases a year, and the application of the test to all other cases at a low rate of payment.

Lastly, it is proposed to introduce a modification of an Act which is in force in New South Wales so as to provide that any person sentenced to any term of imprisonment for any cause can, if found to be suffering from contagious venereal disease, be detained until his release is free from risk to other people.

Such, then, are the steps which have been taken, and which, as may be seen, comprised a comprehensive investigation into the extent of distribution of these diseases, and after its ascertainment an attack on medical lines. In this work the clergy have been of great service. They have had the facts placed fairly before them, and have opened their pulpits to the medical men who were prepared to give a temperate statement of fact.

In the course of this interesting but at the same time unpleasant work the writer has been faced with two self-imposed questions which have pressed him hard for an answer. Why in rich, prosperous Australia, where female domestic labour commands so great a remuneration and is always in demand, does any woman wish to be a prostitute? Poverty is not the cause, yet prostitutes are probably as numerous as in many other countries. Why is it that mothers do not encourage their daughters to marry decent young men who attract them, even if their income is small? Surely the artificial standards set and the consequent late age of marriage are the root cause of most of this social excrescence.

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Perhaps the plain speaking in this article will shock some readers. But the facts narrated are the facts of life, and a civilisation which tries to cloak such facts when their disclosure is necessary for betterment is to an extent unsound and artificial. There is a proper place, time and manner for discussing the pathological and seamy side of society, and I hope that readers will realise that the statements made are temperate and warranted, and that the gravity of the matter justifies the publicity now given.

During my recent visit to Great Britain I found that a belief in the decline of syphilis was held in some quarters. There is no doubt that, owing probably to better treatment and better hygiene, the grosser manifestations are not so common. The deadly form of disease known as submerged syphilis is probably just as frequent, since the distribution of nervous syphilitic diseases is not decreasing. But by the use of the Wassermann method on a large scale the question could be settled. If, for example, every patient admitted to Brompton Hospital were examined for a period, the relation of syphilis to tuberculosis could be settled. We should then know how far tuberculous infection is based on syphilitic malnutrition.

In conclusion, I think that syphilis is far more common in European cities than in Australia. We are inclined to hazard the guess that in Melbourne it must affect 5 per cent. of the population.

From the foregoing it will be seen that the elimination of venereal disease is more than possible. It is, in fact, not a very difficult matter once it is faced straightforwardly. It would immensely reduce the amount of unjust suffering and misery in the world. A knowledge of the facts may not simply end in the extirpation of the diseases, but may pave the way for the gradual recasting of social relationships on healthier and better principles.

THE MILK PROBLEM:

The Supply

By Eric Pritchard, M.A., M.D.

IN the January number of *BEDROCK*, after reviewing the arguments for and against the sterilisation or pasteurisation of milk, I came to the general conclusion that, except in one comparatively unimportant and easily remedied particular, these procedures in no way impaired the digestibility or the nutritive properties of the raw product. On the other hand, I adduced evidence to show that the risk of infection with diseases of both human and bovine origin is real and serious if milk is consumed in the raw state. As further emphasising the danger of milk infection, I would now add that during the years 1907—11 there were five serious milk epidemics in the city of Boston involving risks to the lives of 4,095 persons. These epidemics took the forms of diphtheria, scarlet fever, enteric fever and sore throat respectively. An examination of the epidemiological records of many of our own towns proves that in this country equally serious indictments can be urged against the use of raw milk. No precautions short of killing all pathogenic germs, should such be present, by heat or other bacteriocidal means, can make raw milk perfectly safe. Further, no man can guarantee that any particular sample of milk has not been thus exposed; indeed, quite a serious outbreak of scarlet fever occurred some little time ago in America among persons who consumed certified milk of the highest quality, obtained from a seemingly unimpeachable source. On the other hand, as far as I am aware, no case of infective disease, much less an epidemic, has ever been traced to the use of pasteurised milk.

If the heating or pasteurisation of milk can thus ensure immunity

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from the dangers due to its accidental contamination with the germs of disease, it may well be asked why should not all milk be so treated before consumption. One of the arguments is that this procedure can effectively cloak dirty production and careless distribution. Dirty milk is always teeming with bacteria, and within limits the number is proportional to the age of the milk, the temperature at which it has been kept and the amount of dirt present. The more numerous the bacteria the sooner does milk become sour; but when milk has been pasteurised it does not readily turn, and the public thereby may be deprived of a useful and practical criterion of the extent of the contamination. The same argument, however, could be advanced against the practice of refrigerating milk immediately after it has been drawn from the cow. This danger, namely, that the pasteurisation of milk may cloak contained dirt, is more theoretical than real, for stale and dirty milk, in spite of pasteurisation, would betray itself to the palate and would easily reveal itself in a laboratory examination by the number of moulds and liquefiers, and by other evidences of contamination present in the sample.

Without being aware of the fact, Londoners consume a considerable quantity of milk which has been pasteurised, perhaps more than once, and although possibly there is no valid objection to this treatment as far as the food value of the milk is concerned, in all justice the consumer ought to be informed of the fact, for raw milk unsoured is a higher grade product than pasteurised milk which still appears to be fresh. It must be remembered, however, that efficient pasteurisation greatly increases the cost of production, especially to the small dairyman with a limited business. The best modern plant is very expensive, while increased standing costs for rent, rates, taxes, labour, fuel, power, refrigeration, and a greatly increased charge for bottles necessarily make pasteurisation a very costly procedure. It is the apprehension of these facts that makes the average dairyman oppose the practice of pasteurisation. If the pasteurisation of milk comes into general vogue it will inevitably necessitate the concentration of the dairy business into a few hands, and this would doubtless be to the benefit of the public, for the cleanly production of milk must entail constant supervision and inspection, and it is more economical for the Public Health Authority

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to keep a watchful eye on a few large businesses with great interests at stake than on a large number of small businesses in which only small interests are involved.

The actual quantity of dirt which accidentally finds its way into milk, between the time of milking and the time of consumption, is very considerable, unless special means are taken to exclude it. The actual amount can be estimated in various ways ; the dirt can be collected and measured after subsidence, after filtration, or after centrifugalisation ; but the results thereby obtained must be regarded as approximate only. Some few years ago Professor Delépine calculated that the amount of slime or dirt supplied daily to Manchester in 40,000 gallons of milk was 106 lbs. It has been estimated that Chicago annually consumes 25 tons of dirt in its milk supply, while Orr has determined that the average amount of dirt-sediment in milk, derived from unclean cows, is about 52 parts per million of milk. He found, however, in the milk from clean cows only 31·7 parts per million of milk. To further emphasise how easily preventible a large proportion of this dirt is, it may be mentioned that by quite simple precautions the amount of dirt in the Manchester milk was reduced to 78 lbs. per 40,000 gallons by the year 1906. Practically the whole of the slime found in milk is produced in the mammary gland of the cow itself, and therefore not preventible by improved dairy technique, though possibly preventible in part by special selection of the cows and by careful attention to their health ; on the other hand, it is quite possible to prevent extraneous dirt from finding its way into the milk provided sufficient care is taken in the milking and subsequent handling of the milk.

A very striking method of demonstrating the comparative cleanliness or the reverse of different samples of milk is to filter a pint of each through little discs of cotton wool ; a comparison of the resulting deposits will afford a very accurate indication of the amount of dirt contained in each sample. The accompanying illustration (Fig. 1) shows the quantity of dirt, mostly barn-yard manure, collected by this filtration method from two samples of milk. The milk for these experiments was purchased from milkmen who were delivering milk in the ordinary way at the doors of well-to-do families, living in the most fashionable quarters of London. In each case the milk was supplied by well-known and reputable dairies. The second group of

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illustrations (Fig. 2) shows practically unstained discs through which has been filtered one pint of nursery milk, which has been carefully handled and which is sold in bottles at the rate of 8d. per quart. The dirt represented in the first group of discs, though consisting

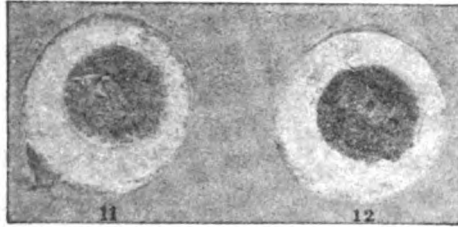


FIG. 1. Milk purchased from well-known London Dairies.

chiefly of dung, earth, hair and vegetable fibre, is not necessarily detrimental to health, even in the uncooked condition, but it is dirt of a character which persons of refined taste would be unwilling to swallow if they knew of what it consisted.

The truth is that very few people care to enquire into the con-

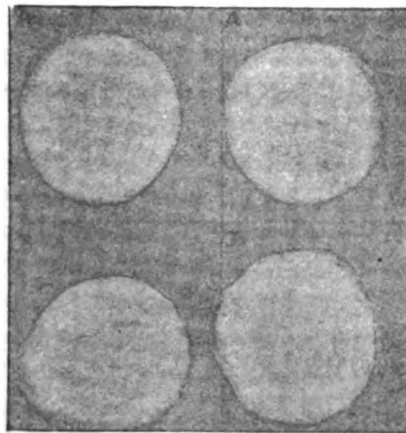


FIG. 2. Nursery Milk.

ditions of the milk supply, and to show how very little Londoners know about the natural characteristics of milk, it is only necessary to instance the common practice among London dairymen of artificially colouring milk with annatto or some other dye. It is surely a serious reflection on our intelligence that this psychological fraud

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should be perpetrated, to induce us to believe that the milk is richer than it really is. As a matter of fact, dairymen colour their milk on a chromatic scale adjusted to the varying degrees of understanding or gullibility of their *clientèle*. When I was a medical student living in a humble quarter of Paddington, the milk I drank was of a light shade of ochre. What I drink now is of a pale primrose colour. If a dairyman sells milk of the natural colour he is threatened with the analyst or the police.

This device of the dairyman conjures up in the mind of the consumer pictures of Alderney cows knee-deep in buttercups, and rich churning qualities of the milk; it constitutes, however, the same sort of *suggestio falsi* as that which prompts the East End confectioner to place fresh lemons in close proximity to the abominable synthetic compound he sells as lemonade. But when ignorance in such matters obviates uneasiness of mind, it is perhaps folly to enquire too closely into the horrors that may lurk in primrose-coloured milk. We have had our pleasures in strawberries and picnic tongues ruthlessly sacrificed by well-meaning food reformers, and now, if we listen to all that is told us by the inquisitive bacteriologist, we shall have no further faith in milk, butter, cream, cheese, or in any other reputedly wholesome dairy product. The pure food conscience is not an unmitigated blessing by any means.

As a matter of fact, it would not require any great effort to produce clean milk, and the enhanced price at which such milk would sell would well repay the producer—provided, of course, there were an adequate demand. To produce clean milk the cows must be well groomed before milking, and the milkers must have clean hands and clean overalls; by these simple means the greater part of the dirt which usually falls into the milk can be excluded. There are many other refinements which still further reduce the risk of contamination, as, for instance, the milking of the cows in concreted barns. the use of special milking pails, and the scientific cleaning of all vessels, utensils, filters, and coolers, employed in the handling of the milk. The rapid cooling of the milk after milking, and the maintenance of a low temperature are essential conditions of a low bacterial count. Although it is possible to produce milk free from adventitious dirt by these and similar precautions, it is not

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possible to produce milk which is free from bacteria, for bacteria practically always exist in the so-called cistern of the mammary gland of the cow ; on an average milk, as it issues from the udder, contains 200 bacteria per cubic centimetre ; the cleanest results have been obtained by Mr. S. L. Stewart, of Brookside Farms, Newburgh, New York, who succeeded, by adopting special precautions, in producing milk almost exempt from bacterial contamination.

It has been proved that the washing of the udders, belly, flanks and tail of the cow before milking can reduce the number of bacteria which fall into the pail during the course of two minutes, from 4,752 to 230, while the mere washing of the udders can make a difference of 6,342 bacteria per cubic centimetre of milk. These results are hardly surprising when we remember that on a single hair of a cow can range some 27,000 bacteria, while dried barn-yard excrement can contain 13,050,200,000 per gramme. Imperfectly cleaned milk coolers and unsterilised receptacles for the milk are prolific sources of bacterial contamination. But however great may be the number of microbes which fall into milk between the time of milking and the time of consumption, it is insignificant when compared to the number which can be produced by germination in the milk itself when the conditions are favourable. For instance, if clean milk containing only 3,000 bacteria per cubic centimetre is kept at a temperature of 68° F. for 24 hours, the bacterial content may be 450,000, and if kept for 24 hours longer, the number may amount to 25,000,000 per cubic centimetre. While in less clean milk, initially containing 30,000 bacteria per cubic centimetre, there may be 4,000,000 and 25,000,000,000 after similar intervals of time. Till quite recently the rapid multiplication of germs in milk during hot weather was prevented by the addition of antiseptics, but now that this practice has been made illegal by a Local Government Board regulation we may expect to meet with a considerable quantity of sour milk during the hot days of summer, unless dairymen adopt the method of pasteurisation.

If, then, the production of clean milk is merely a question of dairy technique and cost, there ought to be some system of grading and labelling milk according to its standard of cleanliness, in order that those who prefer to drink clean milk, and are prepared to pay for it, may have some sort of assurance that they are obtaining what they

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pay for. The grading of milk is not altogether a simple process ; the filter test is by no means reliable, for the dairyman can filter the milk before he delivers it at your door, while the *desideratum* is milk into the composition of which barn-yard filth has never entered, not milk from which such dirt has been removed by mechanical means. The method of the bacterial count is more trustworthy, for it is not practical to filter away bacteria from milk, and the number of bacteria present in milk is a more or less reliable index of the degree of dirt contamination. There are, however, many fallacies connected with this test also ; for instance, the milk produced under the most filthy conditions will contain very few living bacteria if it has been pasteurised or otherwise treated by heat whereas milk produced under ideally clean conditions will show a very high count if the milk is kept for a sufficient number of hours at a favourable temperature. If, therefore, we employ the bacterial count as an index of the cleanliness of the milk, we require also to know how long the milk has been kept, and at what temperature. We should also be informed whether the milk has been submitted to any process of sterilisation which may have killed its contained bacteria.

In America milk which contains less than 10,000 bacteria per cubic centimetre, which is derived from non-tuberculous cows, and which has been produced under conditions of approved cleanliness, is graded as "certified milk" and is sold at prices which range from 10d. to 1s. per quart. Certificates for this high-grade milk are granted by *Medical Commissions*, which consist of medical men who appoint bacteriologists and veterinary surgeons to carry out the necessary tests and to make periodic inspections of the cattle, farms, and dairies. If at any time the milk does not come up to standard the licence to sell "certified milk" is withdrawn. Although milk of this high standard can be obtained in London at the price of 8d. a quart, there is at present no guarantee, beyond the good faith of the vendor, that such milk is of the standard claimed. As the law stands, any dairyman can sell milk in bottles with the words "certified milk" printed on the labels, although the milk may have no claims whatever to such a designation. As a matter of fact, there is no little deception practised in the selling of bottled milk. Bottled milk may be a delusion and a snare ; it does not

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improve the quality of an already dirty milk to pour it into improperly cleaned bottles and sell it as some specially pure milk under some striking designation. It is possible that lack of confidence in the assurances of dairymen with respect to the quality of the milk is the reason why the sale of high-grade milk is so restricted. But if clean, properly pasteurised milk is delivered in efficiently washed and sterilised glass jars there can be no question of the superiority of such a method of delivery over that now customary in this country. As the delivery of milk in this manner is only now coming slowly into vogue it would be a great gain if proper requirements in this connection were to be immediately formulated before their enforcement could be opposed as a hardship. Although I think I could state fairly accurately the number of bottles of milk of the quality of "certified milk" sold in London daily, I refrain from doing so for very shame. Milk of a slightly inferior grade to "certified milk" is sold in America under the designation of "inspected milk"; the price is considerably less, ranging from 6d. to 8d. per quart. Such milk must be derived from non-tuberculous cows, show a count of not more than 100,000 bacteria per cubic centimetre and be produced under approved conditions. Milk of lower grade than this is sold as "market milk," or "pasteurised milk." In its raw state such milk should not contain more than 1,000,000 bacteria per cubic centimetre (New York), 500,000 per cubic centimetre (Boston), 100,000 per cubic centimetre (Rochester).

In Berlin 20,000 litres or more of so-called "nursery milk" are sold daily by the great firm of C. Bolle. The milk is delivered in bottles, costs 40 pfs. per litre (6d. a quart), and it is derived from non-tuberculous cows, while the farmers who supply the milk are subject to periodic inspections by veterinary surgeons and other agents appointed by the company. The standard of cleanliness, as estimated by the bacterial count, is almost as high as that required for certified milk in America. More than half the samples of nursery milk show less than 5,000 bacteria per cubic centimetre.

The great bulk of this firm's business is not the supply of "nursery milk" but of clean milk of about the same standard of purity as inspected milk in America; the price is 24 pfs. per litre (about 3½d. per quart). All milk of this quality is pasteurised for twenty minutes at a temperature of 60° Celsius, and the quantity sold is

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nearly 150,000 litres per diem. As an indication of the magnitude of this firm's business it may be stated that 2,400 men are employed in the industry, and that the milk is delivered in 320 carts. In Berlin the advantages of a clean milk supply are appreciated.

In Dresden the famous firm of Pfundt Brothers supplies more than 20,000,000 litres of milk annually. The price varies according to the grading. Nursery milk is sold at 25 pfs. per litre ($3\frac{1}{2}d.$ per quart), ordinary milk in sealed bottles is sold at 22 pfs. per litre, and in sealed cans at 20 pfs. per litre. The chief feature of this Dresden supply is that all the milk, as well as all the milk products, such as cream, separated milk, butter and cheese are pasteurised before sale. This is an important point, for cream and butter are even more liable to contain tubercle bacilli and other disease germs than ordinary milk. It is a common practice in England to boil milk in the home before consumption ; to be logical all cream and butter should be sterilised also.

The public in this country clearly require to be enlightened on matters that relate to milk. In the first place, it must learn that if it requires clean milk it must pay for it, and in the second place it should be aware of the fact that if it requires safe milk it must be pasteurised ; and incidentally I would suggest that it would be desirable that it should know that the colour of milk is white and not yellow. The dairyman and the farmer have also much to learn, but until there is a market for clean milk it is not worth their while to produce it or learn how to produce it, for to supply clean milk at the ordinary prices ruling for dirty milk clearly would not be sound business.

The sooner the public learn that milk must be graded the better will it be for all persons concerned ; it is quite impossible that the sale of milk can remain much longer exempt from the influence of ordinary commercial laws. In all other business transactions there is a relationship between quality and price ; in the milk trade no such relationship is acknowledged. In the grading of milk there is, and probably always will be, a considerable difficulty ; the same difficulty presents itself in arranging a sliding scale of charges to correspond with the varying qualities of the milk. It would, however, be a step in the right direction if in the proposed " Milk and Dairies Bill " the Local Government Board were empowered to fix a standard for

THE MILK PROBLEM

milk sold under the designation "certified milk." For instance, it might be made illegal to sell milk labelled as "certified milk" which contains more than 10,000 bacteria per cubic centimetre, which contains added substances of any kind, including colouring matter, which contains less than 3·5 per cent. of butter fat, in which the solids other than fat are below 8·5 per cent. and which has been subjected to any preserving process other than refrigeration.

"The Milk and Dairies Bill" provides for the registration (licencing) of dairymen as well as dairies, but it does not provide for the annual renewal of the licences. It seeks to obtain powers for the local authority to inspect dairies, examine cows, and prevent the sale of tuberculous milk and to issue regulations for making the Act effective. The success or failure of the Act will clearly depend on the nature of the regulations. If orders can be devised and enforced for the production of reasonably clean milk without materially raising the price, and without unduly worrying and harrying the farmer and the dairyman, an Act designed on the lines proposed should be extremely useful. The Bill specifically claims to issue regulations for the prohibition of the addition of colouring matter to milk, and of the addition to milk intended for sale for human consumption of skimmed milk or separated milk or water or any other substance. It also seeks to enforce the correct labelling or branding of the receptacles which contain milk *not in its natural condition*. This provision means that, under the Act, pasteurised milk shall no longer be sold without a label clearly stating that it has been artificially heated. If this Bill eventually becomes an Act it will enormously improve the quality of the milk supply, and not least because it will educate the public with respect to the present defects and future possibilities of the supply.

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FACTS AND THEORIES, being a Consideration of some of the Biological Conceptions of to-day, by **SIR BERTRAM WINDLE, M.A., M.D., Sc.D., LL.D., F.R.S., K.S.G.**, President of University College, Cork. (Catholic Truth Society.) Pp. 163. 1912.

It is seldom that a book of this type is noticed in such a periodical as **BEDROCK**. This is certainly a pity. For the motives behind and the methods employed in it are not unworthy of dissection. That it originated in a dual purpose is unmistakable. It was designed to serve both as a vaccine and as an antitoxin.

For Catholics in whom the first signs of a budding curiosity as to the trend of modern thought have been providentially detected, it will be used as a vaccine; for those reprehensible ones who have had the temerity to indulge their curiosity, as an antitoxin.

How to shield the minds of its flock from being influenced by the hostile criticism of the day is a problem that has beset the Vatican, in one form or another, throughout its history. Up to the beginning of the sixteenth century the fight had, practically, never extended beyond the confines of the unverifiable. Then, however, with the discovery of marine shells and other fossil remains in the strata of the Italian peninsula, the battlefield was shifted to the realm of *fact*. It was the ushering in of a new era, and none perceived more clearly how momentous were the inferences involved than did the Church herself. She saw at once that, if these were the shells of creatures that had actually lived, Patristic Chronology would have to be abandoned, and the date of the creation of the world (and of man) set back indefinitely, in which case it became appallingly obvious—in view of the rapidly approaching end of the world, then universally believed in—that God must have left the vast majority of mankind to their doom, confining the chance of being saved to those only who should have the good fortune to be born in the few closing millenia of the world's existence. "No," said the theologians, "a God of justice can never have done that, therefore these sea-shell conjectures may be dismissed *in toto* without further consideration."

And then, lo! and behold! there came to their assistance a certain "hard-headed man of science." He happened, as does the author of this volume, to be an anatomist. How far the study of anatomy

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qualified him to speak with authority on conchology and geology is immaterial; for Falloppio was a very distinguished man, and "prestige suggestion"—to use Dr. McDougall's admirable term—was even more potent in its operation then than it is now. Curiously enough, the great Falloppio had originally been intended for the Church, and, indeed, actually held at one time an ecclesiastical appointment in the Cathedral at Modena. That the theologians, therefore, should have found a champion in him is not altogether to be marvelled at. But this is how he did it. He flatly denied that any of the specimens in question were shells at all, declaring that they "were generated by fermentation in the spots where they are found; or that they had in some cases acquired their form from 'the tumultuous movements of terrestrial exhalations.' " *

But other times, other methods. Since the good old days of Falloppio the Church of Rome has learned, as the result of many bitterly humiliating experiences, at least one extremely useful lesson. That lesson is, never to risk coming into collision, now or hereafter, with a *verifiable fact*.

And so, when, after a lapse of nearly 800 years, we find another "hard-headed man of science" (who is also an anatomist) coming to the assistance of the theologians, we see him exhibiting a guarded cautiousness as to positive and final statements of fact that would have driven his celebrated prototype of Padua wild. Nor would the theologians of to-day have it otherwise. In fact, it is only quite a special brand of hard-headed men of science that they have use for. To begin with, he must be peculiarly circumspect and peculiarly docile—so docile, indeed, as to submit with cheerfulness his *scientific* writings for *ecclesiastical* approval before venturing on publication; while lastly, and most important of all, he must (if possible) occupy so elevated a position in the scientific world that, by the operation of "prestige suggestion," his pronouncements will readily be accepted—by the *non-scientific*—at many times their true value.

Happily the author of this work is bounteously qualified in all these respects. For, somewhere between the cover and the title-page of his earlier book, *What is Life?*—an essay on neo-vitalism, of which a considerable proportion has been transferred bodily to the present volume—was found, by a happy accident, a quite unlooked-for page adorned as follows:—"Nihil Obstat, Michæl Maher, S. J. Censor deputatus. Imprimi potest. ✕ Gulielmus Episcopus Arindensis V. G. Westmonasterii, Die 1, Novembris, 1907"—while, besides being an anatomist, the author also happens to have become a Fellow of the Royal Society.

Of course, the plan of campaign of so doughty and, withal, so circumspect a champion as this, was bound to be worth studying. And so it

* Lyell's *Principles of Geology*, 10th ed., Vol. I., p. 33.

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is. No more of stout old Falloppio's frontal attacks for him. Frontal attacks are apt to reveal the strength of the enemy's position ; which is just what the grown-up babes and sucklings, for whom this book was written, must be kept in ignorance of. No, the only way to deal with those terrible fellows, "the Materialists and Monists," was—if we may continue the metaphor—to creep round one of their flanks. And this is just what our author has tried to do—in a fashion of his own. For if, under the circumstances, it was desperately impolitic to attack them—the Materialists, the Monists and the Agnostics—on their own ground of philosophy, then, surely, the most wily alternative, were it only feasible, would be, by hook or by crook, to discredit them as scientists instead, and then leave it to what may be termed "shattered-prestige suggestion" to discredit them as philosophers as well.

With his large experience of the class of minds it was sought to influence, it seemed to President Windle that *by hook or by crook* this wily alternative *was* possible : and hence this book.

THE HERMIT OF PRAGUE.

THE MECHANISTIC CONCEPTION OF LIFE. Biological Essays, by JACQUES LOEB. (University of Chicago Press, Chicago, Illinois, 1912.)

The duty of presenting to the public the results of science in an interesting and intelligible form is perhaps not adequately appreciated by all men of science. And as a consequence, we find on the one hand that the public look askance at science : they misunderstand and are afraid of its iconoclastic propensities, and they do not reap the profit from it that they might : while on the other hand men of science complain of the lack of financial support, and tend to look with contempt upon a public, apparently so indifferent to the furtherance of its own best interests. And hence has arisen the extraordinary fallacy that science is dull and heavy. For whatever dullness or heaviness there may be belongs not to science, but to the abortive efforts of its mediocre expositors, the kind of man described by Goethe—

"Who buries deep for hidden ore,
And when he finds an angle-worm rejoices."

Men do not recognise that, just as we look to music, dancing, etc., as the chief *gaeties* of life, so we should look to science as the chief *interest* of life : far more important and far more interesting than those various political questions, which at the present time constitute the staple subject of conversation among well-educated persons. We may rise from a scientific discourse with the exclamation, "How beautiful a thing is life !" But who has ever arisen with such a sentiment from the perusal of a parliamentary debate ?

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The work of Jacques Loeb is singularly appropriate for popular appreciation : and in the volume now under review that eminent *savant* has himself undertaken the task of expounding his conclusions to the general public. He does not indeed attempt any " explanation of life " on mechanistic lines : he confines himself to indicating the mechanistic or materialistic significance of the conclusions which his own personal researches have led him to, chiefly connected with problems of fertilisation. It was not so very long ago that even so materialistic an operation as the fertilisation of an egg was looked upon as spiritualistic in essence. Till late in the eighteenth century, it was believed that no direct contact between sperm and egg was necessary : but that a spiritual and volatile product of the sperm known as the " aura seminalis " brought about the fertilisation of the egg. The theory was finally disproved by Spallanzani, who put male frogs into trousers during the act of cohabitation, with the result that the eggs remained unfertilised.

Even up to the end of the nineteenth century the field of heredity continued to be " the stamping-ground for the rhetorician and metaphysician " : and it is largely to Loeb's personal work that it has now been definitely reclaimed for physical chemistry. His experiments on the artificial induction of parthenogenesis are too well known to need comment here. He proved that the unfertilised eggs of sea-urchins could be caused to develop to the pluteus stage, by changes in the temperature and salinity of the water in which they were reared. He proved also that the whole process of development might be initiated by the sperm of animals of widely different species, such as starfish : though in these cases the young animal derived its hereditary qualities solely from the maternal side, the sperm acting merely as an instigator of the egg's development, by the mechanical removal of obstructions.

Whereas it is beyond either my knowledge or my desire to attempt a criticism of the details of Loeb's scientific work, it is not without interest to note its relation to the general tendencies of modern biology. Like the Mendelians, Loeb is no believer in the all-sufficiency of Natural Selection. As in their case, his criticism of Natural Selection is founded on objections to the teleological basis of Darwin's great principle. In pre-Darwinian times it was held that every organ, every structure in an animal had immediate reference to some life-conserving purpose ; every structure was considered to be developed for some specific use : purpose was at the basis of morphology. This central conception was in no wise changed by the *Origin of Species*. Natural Selection did not question the fundamental assumption of the purposiveness of life and of structure : it merely presented a materialistic explanation as to how that purposiveness was brought about, in place of the spiritualistic, theological or metaphysical explanations previously current. This modification of men's conceptions about the origin of species was, as history has shown, as great a revolution in thought as could possibly be

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undergone in one generation. But twentieth century biology tends to carry the revolution much further. All the vital movements in modern biology criticise, not so much Natural Selection—for as Herbert Spencer used to say, Natural Selection is seen to be obviously and inevitably true, the moment the theory is stated—but the assumptions lying at the basis of Darwinism, and received by Darwinism without question from the cloudy superstitions of the past. The modern movements are movements away from teleology. They are movements whose explanations do not include the notion of “purposiveness”: but rest ever more completely on the blind and fortuitous operation of natural forces. Mendelism takes no note of “purpose” whatever. Loeb’s work is scattered throughout with attacks upon the teleological elements in Darwinism: such, for instance, as the very common existence of galvanotropism among animals, which is a reaction alleged to be entirely irrelevant to their needs. And in various other quarters the same tendency may be seen. In the new edition, already half issued, of that great work *Brehms Tierleben*, the position is taken up that the brilliant plumage of humming-birds is due to an incapacity of the kidneys to eliminate the excessively abundant excretory products, caused by their prodigious activity, and that these products accumulating in the feathers produce the bright colours, which (according to the above work) have been found to be of little or no value to the life of the species.

I am not here expressing any opinion on these views; I am only noting tendencies. And what I wish to emphasise is this: that, while during the last decade journalists, sciolists, demagogues and popular lecturers at large have been steeplechasing over the country declaring that mechanistic and materialistic biology was dead, the real fact is that every vital movement and every tendency of modern biology is towards a materialism scarcely dreamt of in the nineteenth century.

In many parts of Loeb’s work emerges his distrust of metaphysics. The mistake of metaphysicians is, he says, that they “substitute a play on words for an explanation by means of facts.” In psychology, he lays great stress on the theory of association, in common with all whose standpoint is mainly physiological. He proves that primitive forms of will are mechanical tropisms. As to morals, he states his view in terms which seem to me entirely incontrovertible: “not only is the mechanistic conception of life compatible with ethics, it seems the only conception of life which can lead to an understanding of the source of ethics.”

HUGH S. ELLIOT.

MODERN RESEARCH IN ORGANIC CHEMISTRY. F. G. POPE. (Methuen.)

An excellent book in the hands of the advanced student and of the research chemist. The author deals with the development of modern chemistry in a series of monographs which are arranged in a particularly

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happy way, showing the chronological evolution of each subject, and ending with an index of the literature pertaining to the subject.

The language is very terse and yet clear, and the formulæ are used with conspicuous success being both instructive and explanatory. A careful perusal of the book will familiarise the reader with the subject, whilst being at the same time suggestive and inspiring.

The following subjects have been dealt with :—

The Polymethylenes.

The Terpenes and Camphors.

The Uric Acid or Purine Group.

The Alkaloids.

The relation between the Colour and Constitution of Chemical Compounds.

Salt Formation Pseudo-Acids and Bases.

The Pyrones.

Ketens, Ozonides, Triphenylmethyl.

The Grignard Reaction.

It is to be regretted that some problems, which have been prominent in the minds of chemists for a number of years, and on which great work has been done, have been omitted. Surely the studies of E. Fischer on polypeptides were worthy of a chapter, and the work of Sabatier and Senderens and of their followers on catalytic reactions would have been educational and interesting to the thoughtful student ! Furthermore, there is no consideration of that vast amount of work which is contained in the patent literature, and which is frequently of scientific importance in spite of the practical success achieved thereby. A reference might have been made in the purine chapter to the pyrimidines and purine bases suitable for medicinal purposes. The large number of new anthraquinone rings, dianthraquinonyldiimides, are of scientific, as well as of practical interest. The progress made in the production of indigoid colouring matters must be interesting to every chemist, and would have been appreciated also by the student seeking purely scientific instruction. To the ordinary student they would have brought home the great practical importance of chemistry in every walk of life.

We hope that in a new edition the scope of the otherwise excellent book will be extended in the directions indicated.

THE PRE-HISTORIC PERIOD IN SOUTH AFRICA. By J. P. JOHNSON.
Second Edition. (London : Longmans, Green & Co., 1912.) 10s.

Mr. J. P. Johnson is well known to students of archæology as the author of two books and numerous papers on the stone implements of South Africa. As the present volume is a revised and enlarged edition of a book that appeared only two years ago, it would seem that there is a considerable local interest in the subject. This is a matter for

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congratulation, as there are numerous problems in the archæology of South Africa, and it is quite time that a serious effort should be made to solve them. So far as the study of stone implements is concerned, the first stage is practically passed—that of casual collection of specimens. The labours of numerous collectors have revealed the very widespread occurrence of various types of implements, and our author and Dr. L. Péringuey, the Director of the South African Museum, Cape Town, have classified these finds ; the memoir by Dr. Péringuey in the *Annals of the South African Museum* (Vol. VIII., 1911) is specially valuable on account of its beautiful illustrations. It is perhaps inevitable that South African observers should seek to co-ordinate their local types of implements with those of Western Europe, but at the same time this procedure is to be deplored, as it obscures the issues more than it clarifies them. The typology or morphology, as it is variously termed, of European implements is gradually being elucidated, and it is as yet a moot point how far some of them may be of local significance ; even in Europe each area has to be carefully studied more or less independently of other areas, for premature generalisations are as apt to retard research of this kind as to advance it. The study of implements is essentially palæontological in its methods, and at bottom is a question of stratigraphy. If these considerations apply to Western European archæology, so much the more do they apply to the archæology of far distant countries. There is such diversity of form among the older palæolithic implements of Western Europe that it would not be surprising for many of them to be repeated elsewhere, assuming an absence of cultural continuity. The mere form of the implements may suggest such a continuity, but does not prove it, and to call analogous artifacts from very different regions by the same name merely begs the question. It is one thing to say an implement is of Chellian type, but quite another to dub it as "Chellian."

Even in South Africa we are faced with the problem of "eoliths." Mr. Johnson has collected a large number of the plateau flints in Kent, and has no hesitation in identifying similar stones from Leijfontein, near Campbell village, illustrating his identification by means of rough sketches. He admits that there is still doubt as to the plateau flints being artifacts, and the similarity of the South African specimens is no argument in favour of the latter being genuine artifacts, for, if they could be caused naturally in one place, this could also happen in another, granting the conditions were analogous. The evidence for human workmanship depends upon the character of the flaking and also upon patination. A long training is necessary before a student can be considered as an expert, and, further, many collectors do not possess a scientific mind. Mr. Johnson states that there are no geological data available for determining the relative age of the "eoliths" and other implements.

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The author groups as "Acheulic" all implements "from those that appear to be more primitive than the typical, to those that are certainly more advanced. . . . Among the amygdaliths [as he terms these implements] every gradation is met between the thick Chelléen form with unworked butt, the thinner Acheuléen . . . and the proto-Solutréen." Under the circumstances it does not help matters much to call them "Acheulic implements." He classes in one group West European Aurignacian and Solutrian implements, and states that two finds of "Solutric" stone implements in South Africa "exhibit the complete facies of the West European Solutric assemblage"; the specimens sketched by him do not bear out this assertion, however, since there are several well-marked European types which are not recorded from South Africa—borers and "*pointes à cran*" for example.

What is most urgently needed is more stratigraphical investigation. It is true a certain amount has already been accomplished, but until this work is undertaken by trained persons in a systematic manner the archæology of South Africa will never be put on a firm basis. These remarks, however, are not intended as a reflection upon Mr. Johnson, as it is evident that he is alive to the weakness of the evidence; but it is imperative that the subject should now enter upon this second stage of its evolution.

Mr. Johnson publishes some interesting examples of pecked and engraved petroglyphs, and points out the difference between true Bushman work, or "Solutric" as he terms it, and that of Bantus, which is markedly inferior. He also gives us a few copies of Bushman paintings. In a chapter on "The Pre-historic Bantu," which deals with the evolution of the Zimbabwe types of ruin, he says: "There can be no doubt that these ruins are the work of a Bantu people that in some respects attained a more advanced stage of culture than any of the surviving tribes." This Bantu culture received a setback "by warlike tribes who made a living by killing and robbing the more peaceful and industrious tribes." The essay by Mr. A. S. Kennard, "On the Sequence of the Stone Implements in the Lower Thames Valley," was doubtless added to indicate the lines on which study might proceed in South Africa, otherwise it seems somewhat out of place. The second Appendix, giving an account of a journey by the author across bush country, has very little to do with the pre-historic period, and the figure of two copper rods used as currency is not alluded to in the text. There are two good plates of special types of implements and numerous sketches of local types and of European forms for comparison; there are many other excellent illustrations. If the book serves to stimulate further interest in the local archæology, as it certainly should, it will not have been written in vain, and the descriptive portions of it will prove of value to the student in Europe.

A. C. HADDON.

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SCIENTIFIC METHOD, by F. W. WESTAWAY. (London : Blackie & Son, Ltd.) Pp. xx+489. 6s.

Mr. Westaway deals lucidly and most interestingly with an all-important subject. We have only to examine history to perceive how greatly scientific method or, to speak more precisely, the methods adopted by the learned have differed with time and place, and how entirely the progress of society has depended on the methods employed. In every age, but especially when the general standard of intellectual development is low, the mental tone of society, in all except matters of purely individual interest, is governed by that of people who are reputed erudite and, therefore, wise. During the middle ages the thinking of the learned in Europe was founded on a number of statements which derived their authority from ancient philosophers and more recent divines. Considered merely as thinking it was often admirable. It accorded flawlessly with the premises from which it started. But the premises themselves were not, and could not be, verified. The habit of regarding them as necessarily true, and therefore of ignoring as more or less worthless and deceptive all that conflicted with them resulted in a tone of mind highly confiding in some directions, highly sceptical in others, which may be observed as a survival at the present day in Thibet, many Mahomedan countries, and even in sections of modern Europe. Under such conditions science and society stagnated. The overthrow of this method was the principal work of that great, but secret iconoclast, Francis Bacon, who struck not at faiths, but at the foundations of unquestioning faith, not at beliefs, but at the mental habit which rendered certain kinds of belief possible. But if Bacon destroyed much reputed science, he created little real science. That task was reserved for giants who stood on his gigantic shoulders—Newton, his contemporaries, and successors.

The outstanding feature of Bacon's teaching—so outstanding that all else is shadowed—is that our knowledge of Nature must be founded on actual observations of Nature. As long as we are engaged in description, he furnishes a sufficient guide. Thus, if we perceive that men have heads and describe them as characterised by those structures, all contradictory statements are inconceivable as true. No other facts are needed to establish the statement. But when we seek to interpretate, or discover the relation of cause and effect, his teaching is not sufficient. If we think of any object or event in Nature the antecedent of which is not already known to us, and try to account for it, two or a dozen, it may be a hundred, possible interpretations are almost sure to suggest themselves. Any one of them *may* be true ; only one of them *can* be true ; and it is impossible to know which is true until we have observed a great deal more than the facts from which we started. In explaining, then, it is not enough to found statements on observed facts ; it is necessary also to test these statements by more and different facts and to

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go on testing by more and more facts till every explanation but the right one is rendered inconceivable as true. Suppose, for instance, we hear a sound by the roadside. That observation is usually sufficient for description ; we can compare the sound to some other sound ; that is, we can classify it, or say what it is like, which is the essence of description. But the observation by itself is not enough for explanation. The sound may have been caused by a pheasant, or a weasel, or a poacher, or a ghost ; and before we can be sure of the antecedent we must call into court many other facts culled from past experience or gathered after the event.

Here, then, is scientific method in a nutshell : (1) All statements must be founded on verifiable facts ; (2) If the task be description, we need collect no other facts : those from which the thinking started are sufficient ; (3) If the task be interpretation, the initial observations must be supplemented by more observations, until one explanation is established *and all others negated*. Untiring patience in collecting evidence and in comparing rival hypotheses, perfect fairness and open-mindedness, relentlessness in criticism especially of favoured hypotheses, these are the distinguishing marks of great scientific thinkers. So Copernicus, Kepler, Newton, Faraday, Darwin, and all the very greatest did their work. They left no stone unturned, no rival explanation that was not faithfully and fully examined. Smaller men are often abominably prolific in hypotheses. They leap to conceivable explanations, but they test none ; they leave rival hypotheses unrefuted. If they are industrious in collecting facts, they gather only materials that favour their own notions, and as a rule only materials similar to that whence they started. Limbo is full of such thinkers, and paved with their works. Suggest an alternative explanation to a small man, and he, a dullard or a partisan, will be puzzled, or indifferent, or contemptuous, or angry. Suggest it to the great man, and his busy mind, candid, earnest, laborious, will fasten on it, and presently he will show that you are right, or that you are wrong, or, perhaps even, that you have opened up for him a new and prolific field of thought and research.

All this, set out with abundant detail and illustration, may be read in every good book on scientific method, for example, Whewell's *Novum Organum*, Mills' *System of Logic*, Jevons' *Principles of Science*, Welton's *Manual of Logic*, and, latest of all, Mr. Westaway's *Scientific Method*. The last, like all its famous predecessors, is delightfully lucid. And it is shorter. There are fewer details. Consequently the reader is less likely to miss seeing the wood because of the trees—to miss perceiving that the essence of scientific method is the proving of all hypotheses in such a way that all rival suppositions are decisively negated.

Mr. Westaway begins by discussing "Humanism and Realism"—classics and science. He holds the balance very fairly and realises the

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ineffectiveness of classics in imparting useful knowledge, and of much science teaching as a means to intellectual development. "Classics has thus always this advantage. If the same boys had been placed under equally able science teachers, and had taken up Science under equally favourable conditions, it is at least possible, and some authorities say highly probable, that as regards the development of intellectual power, the results might have been far superior." That bitter saying, "The ignorant classic, the uneducated scientist," has a sting of truth. Much "exact" science teaching is not scientific in the sense that it trains the pupil to employ the right methods, and it endows him with a knowledge of important and fully tested generalisations. It consists merely in a mere cramming of facts of no more value to the recipient when considered in relation to his future life than the precise dates, the lengths of rivers, and heights of mountains with which unhappy school children were formerly surfeited. Consider, for example, the mass of very exact zoological and botanical data which university medical students acquire for examination purposes, and forget immediately after.

When discussing "philosophers and some of their problems," Mr. Westaway declares :—

"The man who sets up as a psychologist before undergoing a thorough training in physiology is a mere quack ; he has no more right to be heard on the genesis of the psychical states, or of the relation between body and mind, than the ordinary advertising pill-maker has a right to be heard on a question of medical treatment."

His analogy would have been more perfect had he declared that "the psychologist who is untrained in physiology has no more right to be heard on problems of psychology than the medical man who is unversed in pill-making has a right to be heard on questions of medicine." Exaggerated claims supported by strained analogies awaken nothing but opposition and senseless controversy. Cerebral physiology is still in its infancy. All that is known is that certain brain-areas are related in some uncomprehended way to certain mental happenings. This knowledge, while extremely interesting and important, especially to practical workers such as surgeons, is as yet fragmentary. But every man knows his sensations and thoughts better than he knows anything else. Mr. Westaway's dictum is equivalent to a statement that we are unqualified for considering our sensations and thoughts till we have a full knowledge of their physical basis. By all means psychologists should make themselves familiar with physiology as far as it is known, just as musicians should make themselves familiar with the mechanisms of their instruments. But, just as it is possible to be a pianist without being a tuner, or a cricketer without being an anatomist, so it is possible to be a psychologist without being a physiologist.

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Certainly a knowledge of physiology does not, as in the case of medical students, necessarily lead to a profound psychology.

Again, Mr. Westaway declares :—

“The great characteristic of scientific method is *verification* at every stage, the guaranteeing of each separate point, the cultivated caution of proceeding to the unknown solely through the avenues of the known. The fundamental difference between the metaphysical and the scientific methods is not that they draw their explanations from a different source, the one employing Reasoning where the other employs Observation, but that the one is content with an explanation which has no further guarantee than is given in the logical explanation of the difficulty ; whereas the other imperatively demands that every assumption should be treated as provisional, hypothetical, until it has been confronted with fact, tested by acknowledged tests—in a word, verified. The guarantee of the metaphysician is purely subjective ; it is the ‘*intellectus sibi permissus*’ ; the guarantee of Science is the ever-present desire to submit all facts to a rigorous verification.”

While it is quite true that metaphysics wanders in a sea of conjecture, and that science seeks to verify the interpretations that lie within her domain, these are not the characteristics which especially distinguish them. They deal with different fields of thought. Our minds perceive certain phenomena. Science is founded on the unverified assumption that these appearances represent real objects existing in real space and time—objects that are observed not created by our minds, that existed before the observing mind was, and will continue after it has ceased to be. Metaphysics discusses the grounds of this assumption. Examine any book of science and it is evident that the things discussed—suns, planets, men, women, plants, animals, engines, fuels, mountains, and rivers—are supposed by the writer to be realities external to and independent of his mind. Examine any book of metaphysics and it will be plain that the writer is endeavouring to ascertain what, if anything, lies behind appearances. As William James said :—

“Every science assumes certain data uncritically and declines to challenge the elements between which its own ‘laws’ obtain and from which its deductions are carried on. . . . Of course these data are themselves discussable, but the discussion of them (as of other elements) is called metaphysics.”

When metaphysics endeavours to verify the very data on which science depends, but which she leaves unverified, it is surely incorrect to praise the latter for accuracy and condemn the former for looseness. The true grounds for condemnation lie in the fact that all attempts to pry behind the veil of phenomena are, from the nature of the case, futile, and that, unless the objective reality of the universe be taken for granted, we commit intellectual suicide. How exquisitely absurd, for instance, is

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the proceeding of a metaphysician who, believing that men are no more than phenomena in his own mind, writes a book to explain to them that they do not exist outside that mind, or that of a scientist (metaphysician really) who, after declaring that science deals only with phenomena, proceeds to discuss the evolution of suns and planets, or plants and animals which, as he supposes, began millions of years before his mind had being.

But, after all, such questions as the present value of cerebral physiology to psychology, and the relation of metaphysics to science are of small importance in a book that deals with scientific method. They are somewhat outside its scope. Occasionally the world produces a man of science, who, by heaven-born genius as it seems, adopts the right method. Newton was such a one, Darwin another. But heaven-born geniuses are rare. Most men are bound by conventions; they follow the fashions of their times and places. If a good method is prevalent, they labour in it and help to achieve good results; if a bad method, they labour in it equally and help to build up a system which, presently, the untrammelled genius, if the dull mediocrities fail to choke him, tumbles into ruin. Hence the very unequal results achieved by the learned in different countries and ages, or in the same country and age in different sciences. But the fact that men follow fashions is proof that they are capable of learning. Even if a man has been trained in a wrong method, he may unlearn it and adopt a better. But he must have an open mind; it is essential that he shall not be so dull and orthodox as to accept without question the conventions of his fellows, nor so stupid as to believe without investigation that the method he habitually uses is necessarily right. Many great thinkers have written on methods of enquiry, and so have elaborated a science which is the most basic of all. Guides, therefore, are not lacking, and of contemporary guides, Mr. Westaway is among the best. No man can *know* that he is using a right scientific method until he has read and thoroughly mastered some such book as the one before us.

By one sign the student may know that all is not well in the science in which he is a worker. Its absence is not an infallible token that all is well (for there may be mere stagnation); but its presence is a certain indication that a great deal is wrong. If there is much and long-continued controversy, if the labourers are divided into sects which year after year and generation after generation obstinately maintain contradictory opinions, then it is certain that some of them are merely guessing, and others are declining to accept their guesses, or that all are guessing and there is chaos, or that some are proving and others are declining to accept proof. We have then certain evidence of defective scientific training. On the other hand, if, while the science continually advances, there is discussion but only short-lived discussion, if men after due examination unanimously accept or reject hypotheses, if the

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great principles of the science become steadily established in the sense that they are no longer disputed, then it is certain that all is well. Discussion and even controversy are not wrong in themselves. They are principal means of ascertaining whether hypotheses accord or do not accord with all the relevant facts and so of reaching agreement between workers. Whoever declines them is almost sure to be conscious of a weak case. He may have faith; he is not likely to have surety. But unending controversy indicates partisanship on one side or the other, or both; a desire to maintain an opinion at any cost and in spite of any facts, not a desire to reach truth at any price, even the sacrifice of cherished opinions. It characterises sectarianism, not scientific endeavour.

G. ARCHDALL REID.

GEOMETRICAL OPTICS, by ARCHIBALD STANLEY PERCIVAL, M.A., M.B., B.C. Cantab., Senior Surgeon, Northumberland and Durham Eye Infirmary. (Longmans, 1918.) 132 pp., $8\frac{1}{2} \times 5\frac{1}{2}$. 4s. 6d. net.

"Primarily intended for medical students" and their examinations, but "practically containing all the optics required by an ophthalmic surgeon," this book is a careful selection from available knowledge. Two good rules for making such a selection are firstly that the regions mapped out should not be nearly co-extensive with the whole knowledge of the selector and secondly that the omissions should be dictated by a sufficient experience. That Mr. Percival is well qualified in both respects even a casual reading of the book sufficiently shows. We may quote a few words from p. 90 in illustration:—

"Just as with reflection at a mirror, when an oblique or eccentric incident pencil is considered, the refracted pencil is astigmatic, and presents the same focal lines with the same spheroid shape between them. All this has been omitted in the diagram for simplicity . . .

"It may be asked, Why are lenses made of this erroneous shape? The answer is that it is impossible to mould glass of the right shape with any approach to accuracy, and grinding by hand to any shape but spherical is a most laborious and difficult undertaking."

Here we have the well-considered omission, and an indication of a ripe experience in two nearly consecutive sentences. Neither instance, taken by itself, is more than trivial: but many such scattered through the book make a strong case. The author is nothing if not practical:—

"If in repairing a bicycle reflex lamp the plane mirror is placed at the focus of the convex lens, will it act in the desired way? No. The light from a distant approaching motor . . . will only return to the source of light, and hence give no warning to the driver of the car" (p. 76).

And at the same time the theoretical work is thoroughly clear and

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sound ; and the use of several elegant graphical methods (especially an ingenious one acknowledged as due to Professor Sampson) renders the book valuable to those who dislike or forget the use of symbols. There are numerous questions and exercises scattered through the book ; a useful list of formulæ collected at the end, and a good index. Diagrams (59 of them, all clear and neat). Altogether a most satisfactory book.

H. H. T.

AN INTRODUCTION TO MATHEMATICAL PHYSICS, by R. A. HOUSTOUN, M.A., Ph.D., D.Sc., Lecturer on Physical Optics and Assistant to the Professor of Natural Philosophy in the University of Glasgow. (Longmans, 1912.) 199 pp., $8\frac{1}{2} \times 5\frac{1}{2}$. 6s. net.

A compendious general survey of mathematical physics, and the outcome of six years' experience with a class of students. The six chapters are on Attraction, Hydrodynamics, Fourier Series and Conduction of Heat, Wave Motion, Electromagnetic Theory, and Thermodynamics. After having worked through them, including the examples appended to each, a student would be very well equipped, though there would be joints in his armour. He might, for instance, be ignorant of dynamics, and he might not know a spherical harmonic or a Bessel's function if he met one. He would have had to acquire elsewhere so much knowledge of physical optics as to recognise the properties of a wave of light when he encountered it suddenly in the chapter on Electromagnetic Theory. But attack him on his recognised armour plates and they should be found sound enough—some of them even bullet proof. Fourier series, for instance, is very well done. Perhaps the fairest way to regard the book is to remember its origin ; it is the amplified notes for a series of lectures. The amplification may be varied or supplemented at pleasure by other teachers who will nevertheless find Mr. Houstoun's book a most useful guide, and a sufficient reminder of what they have said on the various topics. The student will certainly, in these hard times, be glad to find in one book at a moderate price what is usually scattered over half a dozen rather costly works.

H. H. T.

PRACTICAL MEASUREMENTS IN RADIOACTIVITY, by W. MAKOWER and H. GEIGER. (Longmans, Green & Co., London, 1912.) Price 5s.

Messrs. Makower and Geiger have attempted to provide a "laboratory course in radioactivity" based on their experience of the teaching of honours students in the University of Manchester. The only criticism which can be offered of a work by authors so practised in teaching and research concerns the selection of their material ; it is possible that some teachers who judge the book by its title may be slightly disappointed

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by its contents. For it is probable that many of those who have to direct practical classes in physics are prevented from introducing their students to the science of radioactivity only by their lack of acquaintance with the technique of modern physics, and may hope to find in the book before us such a treatise on that technique as is urgently required. They will find in it two chapters on the use of electrometers and electroscopes which contain much of the information which they require and a chapter, more useful to those engaged in research than to those engaged in teaching, on the separation of radioactive elements. But even in these chapters many of the difficulties in mere manipulation which meet those who undertake such work for the first time are left unnoticed. The remaining chapters give a sketch of the main facts and theories of radioactivity and suggest experiments illustrating them suitable for students, but no attempt is made to give such detailed instructions for the carrying out of the experiments as are usually found in a text-book of "practical work." We cannot help thinking that many teachers would gladly exchange some of the more theoretical matter, which is adequately described in other works, for a more elaborate account of the adjustment of an electrometer, the construction of a gas-tight ionisation vessel or the pathology of electrometer keys.

However, even if Messrs. Makower and Geiger's book does not seem to everyone perfect, everyone will agree that it is the best book on the subject. It should be in the hands of every teacher who wishes to instruct his students in the more modern branches of science, and will probably do valuable service in increasing the number of such teachers. The authors' names are a sufficient guarantee that implicit faith may be put in the accuracy of all their statements.

CURRENT RESEARCH NOTES

I.—THE THERAPEUTIC ACTION OF RADIUM.

IN August, 1911, the Radium Institute was founded at the suggestion of King Edward VII. through the liberality of Sir Ernest Cassel and Lord Iveagh. The first report of the work of the staff is now accessible and includes a detailed account of the therapeutic employment of radium in nearly seven hundred cases of disease. Various forms of cancer, rodent ulcer and affections of the skin have been treated by the direct application of radium sulphate spread in known strengths upon flat, shallow trays of German silver to a surface of the body. Screens of metal of suitable thickness are interposed between the applicators and the skin. The duration of the exposure varies; from very short periods of a minute to three minutes, to longer ones lasting for hours, while in some instances the time of exposure has exceeded a hundred hours. It is well known that salts of radium, such as the bromide or sulphate, lose nearly all their activity when heated or subjected to a vacuum. Under these conditions a gas, known as radium emanation, escapes. This can be collected, is known to glow in the dark and capable of condensation to a liquid at the temperature of liquid air ($-200^{\circ}\text{C}.$). Radium emanation contains a series of radio-active bodies, which are derived from the different rays emitted by radium salts. Such emanations collected in glass tubes or metal containers used with appropriate screens of aluminium, silver or lead, which act as filters for the different rays, are also employed in exactly the same way as when radium salts are directly applied with the intervention of suitable screens to various surfaces of the body. Since radium emanation is soluble in water, it is possible to prepare artificial radio-active waters which resemble the natural radio-active waters of various well-known spas in Europe which possess a reputation for the relief of gouty and rheumatic disorders. When dissolved in distilled water or a weak saline solution, water containing the emanation may be administered by drinking or by injection.

New methods of treatment too often hold out a prospect of success, which in the nature of things is impossible of realisation, and hopeless and distressful conditions remain unrelieved or even aggravated. Whatever may have been the views held as to the possible efficacy of radium as a therapeutic measure, we are now in a position to recognise

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both the value and the limitations of the work of the Institute. No minute analysis of the evidence is necessary, nor is this given in the report, which is remarkable in two ways. It is rare to read a report which is presented with such scientific caution, and contains little more than a simple straightforward account of the laborious experimental work carried out at the Institute, for it should not be forgotten that the introduction of any new method of treatment partakes of the nature of an experiment.

Parts of the body exposed to radium at first exhibit no effects, either harmful or harmless. Subsequently, between the seventh and fifteenth day, or even four weeks after exposure, the surface where treated shows varying degrees of inflammation, from a simple redness of the skin to an actual destruction of the part. A very pronounced condition of lethargy is frequently, it might be said invariably, observed about the fourth day of the treatment in those who have received prolonged exposures with large quantities of radium, a condition which passes off within a few days of the cessation of the treatment.

The reactions of the organism to radium are therefore quite positive, and it is well known that in some laboratories a number of cases have occurred in which, after a week or ten days, a painful burning sensation of the skin, followed by desquamation of the superficial layers of cells has been caused by handling tubes containing certain rays of radium emanation.

For full details of the cases treated reference should be made to the report. It is sufficient to remark the advice which is given in quite unmistakable terms that radium should never be urged as a treatment in preference to surgical interference for cases of cancer; and of the cases treated at the Institute, and often with marked benefit, the greater number were of such a nature as to be impossible or unjustifiable for operation. Quite otherwise is the evidence with reference to many skin diseases of a malignant character. For these, for certain other affections of the skin, and for some forms of chronic rheumatism, radium has been employed with marked success, so that for suitable cases no method of treatment should be preferred. Little physiological knowledge as to the effect of radium emanations at present exists, and our information on the physiological and pathological effects of the various rays emitted by radium is behind that which has originated from the work carried out in several of the chemical and physiological laboratories of this country.

II.—VARIATIONS IN BACTERIA.

IN the early days when the science of bacteriology was developing, many observers believed that bacteria could change in form. Indeed this view was held by both Pasteur and Billroth. It was assumed, on

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somewhat insufficient evidence, as probable that the same micro-organism could appear in the shape of a minute sphere, rod or comma-shaped organism. This view of pleomorphism was contradicted by Koch and his school, who maintained that pure cultures of bacteria never varied in form but were permanent. With the progress of knowledge it is now agreed that inconstancy of morphological type and varying degrees of virulence are common features of many micro-organisms. Strains of tubercle-bacilli, of *Bacillus typhosus*, of *coli communis* are now known to exist, which vary exceedingly in both cultural and physiological features. Consequently, certain characteristic features of a given strain of bacteria may, by artificial cultivation, so change that the difficulties of classification become greatly augmented. For example, a number of microbes which will not ferment milk-sugar will develop strains or variants of themselves which will readily do this. With certain pathogenic organisms such as the tubercle-bacillus, Fliedermann has shown that this when inoculated into turtles loses much of its virulence for mammals, so that possibly a strain of this may prove to be an effective vaccine for tuberculosis.

Quite recently D. Embleton and F. H. Thiele have demonstrated that it is possible to transform non-pathogenic bacteria into pathogenic forms. A general septic injection, such as is seen in cases of blood-poisoning, can be produced by non-pathogenic bacteria, such as *B. mycoides*, *smegmæ* and *phlei* when their inoculation succeeds the injection of dead homologous bacteria. Of more interest are those experiments where harmless microbes, such as *Bacillus cyanogenus* *Sarcina lutea*, or *Proteus zenkeri*, which even in enormous numbers produce no symptoms when injected alone become pathogenic when injected with solutions of salt water. The essential interest of these experiments, however, lies in the fact that under these conditions the bacteria themselves became pathogenic, as was shown by injecting these alone into other animals.

In the case of *B. mycoides*, this organism, as is well known, will not grow at a temperature of 37° C. Just as such a simple protozoon like amœba, which lives in fresh water, can be educated to thrive in salt water, so *B. mycoides* can be gradually brought to grow at a temperature of 37° C. An animal rendered sensitive by an injection of dead *mycoides* was found to succumb with symptoms of general blood-poisoning when injected with non-pathogenic *B. mycoides* cultivated at 37° C. The organism isolated from the animal is found to present two new features. From the morphological view the organism had changed in shape, having lost its characteristic flagella and developed a capsule such as is found to be present in several common pathogenic bacteria. Moreover, the organism was actually pathogenic and capable of originating a fatal septic poisoning in various animals.

GEORGE A. BUCKMASTER.

NOTES ON NEW APPARATUS

Continuous Infusion and Proctoclysis

IN the history of every great profession, methods are constantly being introduced, which from time to time cause more or less of a revolution. After muddling along for many decades with the transfusion of blood from one person to another, it was realised that it was not the actual corpuscular elements of the blood that were required, but rather the fluid as fluid, with certain properties in it. The transfusion of blood, as such, was discontinued, and an infusion of a watery substance injected instead. These injections were at first made intravenously: a method still widely practised, although it has its limitations, for it necessitates a small operation which is not always devoid of grave danger.

The normal saline, as this artificial blood is called, can be injected subcutaneously in the groins, flanks, breasts or axillæ; but in practice the absorption of the infused saline by this method is slow and uncertain. A very ingenious American surgeon suggested and demonstrated the practicability of the continuous administration of normal saline by the bowel. To this method has been given the name of "proctoclysis." Its successful use is within the power of any careful and intelligent nurse.

Under this treatment some patients who formerly would have died, recover; and those who are going to die, instead of being miserable and tormented by thirst, become more comfortable and have a feeling of warmth and life.

Perhaps the most successful application of this method is for peritonitis, to which these remarks chiefly refer. Whilst a patient is having continuous rectal saline there need be no anxiety as to the amount of food taken. Even should that be *nil*, the rectal infusion is all sufficient until the critical period be past. It is astonishing how large a quantity of the fluid a patient can absorb; even as much as nine quarts in twenty-four hours, or fifteen quarts in three days, without discomfort.

In consequence of the great advantages of this method, doctors and instrument makers are always striving to improve and simplify the apparatus, so that the actual cost may not be prohibitive. Methods usual in hospital are generally too complicated to be successful elsewhere. One of the best apparatus is that invented by

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Dr. Souttar. In it the "Thermos" flask is called into requisition to prevent the temperature of the normal saline falling too low to forbid its entrance into the body. This costs £2 5s. Another useful apparatus is that of Patterson, in which electricity, now so generally installed, is used for the purpose of maintaining the proper temperature of the fluid until it enters the body. Moynihan's apparatus is one of the most simple and least expensive, but requires, perhaps, more skill and attention on the part of the nurse. The bag invented by Lane is the least expensive, requires the least attention, and is in practice the most simple of all the forms of apparatus now used.

Pexuloid Splints

In the last issue of BEDROCK there was an annotation in this column, regarding celluloid splints. The word "celluloid" is misleading. The material used for these splints is non-inflammable, and in order to prevent misconception, and the consequent limitation of its use, the trade has decided to call it "pexuloid."

A New Material for Belts and Jackets

It seems wrong that the maker of steel instruments should also provide belts and jackets; one might as well expect the grocer to supply boots. A company has recently introduced a material for the manufacture of corsets which bids fair to change this. The material, "Spirella," is stated to be washable, unbreakable and rustproof. It is guaranteed for a year, retains its shape, and yet is perfectly flexible. So far these assertions seem to be correct, and to these might be added an important observation. "Spirella" allows free ventilation, a very desirable property in surgical jackets, which makes it compare favourably with other materials, such as leather and pexuloid, which are used for a similar purpose. In fact, as a general rule, the stronger, more rigid materials are liable to be less hygienic. At present "Spirella" is almost solely used for the manufacture of corsets, but it may become exceedingly useful for abdominal belts, spinal jackets, and numerous other purposes.

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BEDROCK

A QUARTERLY REVIEW OF SCIENTIFIC THOUGHT

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1. THE RATIONALE OF PUNISHMENT. By Sir BRYAN DONKIN, M.D., F.R.C.P.
2. THE NEBULAR HYPOTHESIS: III. By Professor H. H. TURNER, F.R.S.
3. THE STRUGGLE FOR LIFE IN TROPICAL AFRICA. By G. D. H. CARPENTER, B.A., B.M., Member of the Royal Society's Sleeping Sickness Commission.
4. MIMICRY AND THE INHERITANCE OF SMALL VARIATIONS. By Professor E. B. POULTON, F.R.S.
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6. LANGUAGE, ACTION AND BELIEF. By J. CERIDFRYN THOMAS, B.Sc.
7. THE TRANSMUTATION OF THE ELEMENTS. By N. R. CAMPBELL.
8. ON THE CONTROL OF VENEREAL DISEASE IN ENGLAND. By J. ERNEST LANE, F.R.C.S.

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NOTES ON RESEARCH.

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- "MODERN MATERIALISM," by W. McDougall, M.B., F.R.S.
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- "ON TELEPATHY AS A FACT OF EXPERIENCE: A REPLY TO SIR RAY LANKESTER," by Sir Oliver Lodge, F.R.S.
- "ON TELEPATHY AS A FACT OF EXPERIENCE: A REJOINDER TO SIR OLIVER LODGE," by Sir E. Ray Lankester, K.C.B., F.R.S.

- "THE NEBULAR HYPOTHESIS AND ITS DEVELOPMENTS: I.," by Professor H. H. Turner, F.R.S.
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- "INTERACTION BETWEEN PASSING SHIPS," by A. H. Gibson, D.Sc., A.M. Inst.C.E., Professor of Engineering in the University of Dundee.

- "THE STARS IN THEIR COURSES" (being substantially the Halley Lecture for 1911), by H. H. Turner, D.Sc., D.C.L., F.R.S., Savilian Professor of Astronomy in the University of Oxford.
- "SOCIAL AND SEXUAL EVOLUTION," by The Hermit of Prague.
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A QUARTERLY REVIEW OF SCIENTIFIC THOUGHT

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JULY, 1913.

VOL. 2.

THE HEAD-MASTER OF ETON AND THE NEW MYSTICISM

By the Hermit of Prague

It would have been interesting, on the eighth of May last, to have watched the features of a British paterfamilias and man of the world, while he read *The Times*' account of the suit brought by Mr. G. Maryan Iyer against Mrs. Besant, the president of the Theosophical Society, for the custody of his two sons, J. Krishnamurthi, aged seventeen, and J. Melyananda, aged fourteen. The inevitable crescendo of facial expression would certainly have reached its culmination during his perusal of the statements made by the lads' tutor. And this would not have been surprising.

"In cross-examination the witness said that he had been conducting certain clairvoyant experiments with Mrs. Besant; he had heard her call him 'a man on the threshold of divinity.' He had seen things in Mars and Mercury. It was true he had stood face to face with 'The Supreme Director of Evolution.' In order to escape the effect of thought-forms and certain astral aspects he had given to two or three boys the advice he was alleged by witnesses of the plaintiff to have given. . . . He had been entrusted with the work of training theosophical aspirants by the super-human beings who were the real teachers of the theosophists."—*The Times*, May 8th, 1913.

Now if, when paterfamilias had read as far as this, it had been possible to switch him on to the article by the present head-master of Eton that appeared this year in the January number of the *Contemporary Review*, it is by no means a certainty that the ensuing changes in his expression would have been entirely anti-climatic.

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For in his new venture he would have had to encounter such unexpected items as the following :—

“ We have to exchange our psychical belief in Jesus for faith in the Christ of ourselves, if we are to enter into possession of the order of knowledge which gave Jesus his power over matter.”

“ If instead of asking ‘ Why has God made such a world ? Why does God allow evil ? ’ we enquire whether God *has* made such a world, and whether God does allow evil, we come to an answer which relieves us of any necessity for a scapegoat. We no longer shift the responsibility of our mistakes on either God or devil, when we understand the laws of our own mind ; for we learn that we have but to develop our intelligence enough to work with the Mental Laws, instead of against them, to bring about incalculable changes in the plane of sensation, which is the final term of thought.”

“ ‘ The riddle of the painful earth ’ is solved as soon as the cosmic memory is recovered.”

The above, it is true, are not Canon Lyttelton’s own words. They are quoted by him from one of the numerous wholly unscientific works on mind-healing that, for several decades, have gained so curious a vogue in the United States, and if his purpose in quoting them had simply been to pillory them, there would have been nothing more to be said. But this, as will be shown later, was *not* his purpose.

The book he quotes from is entitled *Meditation and Health*, and its author, Miss Curtis, is introduced by Canon Lyttelton to the readers of the *Contemporary Review* as “ the leader of the school who practise the way of silence as a means of regeneration.” Moreover, it was with the express and avowed object of letting Miss Curtis explain the nature of the New Mysticism (her own especial bantling) “ in her own words ” that Canon Lyttelton selected the quotations in question.

His own encomiastic attitude towards this New Mysticism may be gathered from his own words :—

“ The teaching is redolent of the recognition of the great unseen forces : the finite is set in a relation to the infinite which, to many unthinking minds, will seem like a new revelation : and finally we cannot help welcoming the fervid propaganda for the need of meditation and silence in a busy and superficial age.”

“ It would not be too much to say that its power of healing character is quite as remarkable as that of healing tissues.”

THE NEW MYSTICISM

Now this is about as large a meed of praise as one could reasonably expect a dignitary of the established Church to bestow on the work of a lady, who, as that dignity himself says, "claims throughout to proclaim a new message to mankind, as much in advance of Christianity as Christianity was of the Mosaic religion" !

Now it is, perhaps, just possible that, even after having enjoyed the privilege of studying Miss Curtis's "own words," the reader's conception of the New Mysticism may still be somewhat lacking in definition. This, however, should it be the case, can be easily remedied : for the head-master of Eton has fortunately favoured us with some elucidatory observations of his own. Here they are :—

"The main view is that Spirit is all-powerful and physical obstacles practically nothing, if only they are recognised as such ; and in practice everything depends on human beings training themselves to live in the permanent influence of this conviction. But, both in practice and in theory, the corollaries from the central doctrine are very numerous and interesting. The sub-conscious mind, as distinct from self-consciousness, is the source of our true communion with the infinite. Man makes his own environment, including his own body : no task, no endeavour is too great for the power of the spiritual man, who has risen to the consciousness of his ' Spirit, Soul, and Body as a Divine Unity.' "

"The practical method of recovering 'the cosmic memory' is meditation, strictly practised day by day for years."

There is now sufficient material before us to justify the question : Which would have surprised our paterfamilias most—the theological tutor, or Eton's head-master ? But no : life is too short for problems of the "Lady or the Tiger ?" order.

Now let us suppose our father of a family, besides being (as stated) a man of the world, to have been a *man of the universe* as well. But first, and with lightning haste, let the most perfervid assurances be given that the term "man of the universe" is here employed with *no* occult or esoteric signification whatsoever. "Man of science," unfortunately, would not serve ; its meanings are so manifold. Broadly speaking, a "man of the universe" may be regarded as one who has built up for himself a robust and reasoned faith in the Uniformity of Nature ; has acquired a sufficiently wide acquaintance with the *order* of her happenings to be able to dispense with the water-tight compartment system of knowledge-storage ; and has become incapable either of "the will to believe" when the

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evidence is insufficient, or of "the will to disbelieve" when it is conclusive. Some day, perhaps, in the dim and distant future, "man of science" may come to mean the same thing.

But now, having, it is hoped, made it tolerably clear what the supposition as to our paterfamilias being a man of the universe as well as a man of the world was really intended to signify, let us try to imagine what that doubly qualified person would have thought of the following remarks by Canon Lyttelton anent the mental healing of a broken leg :—

"It is probable that much criticism will be directed against the statements and claims of the New Mysticism with regard to healing. . . . In particular it will be asked if 'these people' think they can mend a broken leg. A large number of critics seem willing to allow that all kinds of spiritual healing of 'nervous disorders' . . . are reasonable and possible . . . but if any claim to heal a broken joint is advanced, or even hinted at, the whole movement is suspected of fraud. . . . Such strictures, however, are unjust in so far as they deny the logical application of principles generally admitted. Among those who cavil at the New Mysticism . . . are Theists, who ascribe omnipotence to God, and then confidently and scornfully draw the line, limiting His beneficent power to certain phenomena which they think they safely classify as 'nervous.' But the use of this adjective is puerile. All diseases, or anyhow all physical suffering, have something to do with the nerves; and it is just as much or as little of a marvel that the Deity should act on them through the mind, as that He should, through the mind, act on what we call tissue. And why should those who see no difficulty in allowing this last process to be a fact, laugh at the theory that He should similarly act on bones?"

Comment that would do the foregoing justice is forbidden by considerations of space. Suffice it to say that, by a parity of reasoning, to call in question the veracity of the famous inscription in the temple of Asclepius commemorating the miraculous healing of a broken vase would render one chargeable with "confidently and scornfully limiting His beneficent power to certain phenomena"; and to ask how the Church of England could have been guilty of such gross impiety as to dictate to the Deity the means He should employ for the accomplishment of, at any rate, one particular end. In other words, why, by all that is logical, should the Church pray for rain when what is actually longed for is an abundant harvest? Surely, if Canon Lyttelton's argument is sound, it is for Omnipotence

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to determine whether the appearance of the kindly fruits of the earth should be preceded by rain, or not !

But now let us suppose that our paterfamilias to have had a son at Eton. How would that have affected him ? The best answer that can be given to this, perhaps, is to make the following imaginary extracts from his equally imaginary diary :—

“ May 8th.—It may be fairly argued that the thousand odd boys now under L. are, *on the average*, the most precious pack of youngers in the British Empire. It's therefore of imperial concern that as many as can be *humanly* should be set on the right road. This would mean an appreciable crop of better legislators, better administrators, better statesmen in the next generation : and how badly they will be wanted heaven only knows ! If Alexander, the Great, had had Plotinus, the mystic, as his tutor (a fig for anachronisms) instead of Aristotle ; it's odds he would never have set foot outside Macedon ! Then there's that other business. Why on earth don't civilised human beings become physiologically and conventionally marriageable during, say, the same twelvemonth ? It would save a monstrous deal of trouble. As things stand, however, about the chief anxiety of those entrusted with the nation's adolescents is how to get through those unlucky intervening years in safety. If L. would ask the members—every one of them—of the Royal College of Physicians whether ‘ meditation, strictly practised day by day for years,’ would contribute towards that safety—well, I'd just like to watch him reading their replies ! Anyhow, for the thousandth and first time since this morning, *Que diable allait-il faire dans cette galère ?* and for the millionth and first, *Floreat Etona !* ”

HAVING been favoured with a sight of the above article, I am sorry that I cannot reply to the expressions contained therein on what I wrote. There is nothing to reply to. The writer has not seen that I was trying to say what I could in favour of teaching which seems to me, from the philosophical point of view, nonsense ; and he makes things worse by attributing to me opinions which were simply given as a digest of Miss Curtis' own words. Most of the article, however, has nothing to do either with Miss Curtis or me, and only after careful reading I discovered these paragraphs are intended to be jocose. Whatever else may be thought about the subject called “ The New Mysticism,” there is no doubt it is difficult, and I daresay both your contributor and I would be the better for some “ Meditation and Silence ” before we write any more thereon.

ETON, June 14th, 1913.

E. LYTTETON.

MENDELISM, MUTATION AND MIMICRY

By Professor R. C. Punnett, F.R.S.

To the naturalist and the biologist few conceptions have appealed with greater force and charm than Bates' theory of mimicry : few also have been more ingenious than Fritz Müller's extension of that theory. Warmly welcomed by Darwin, rapidly developed by Wallace, Trimen, and not least by Professor Poulton himself, the facts grouped around this heading have come to be regarded as one of the strongest arguments for the theory of specific change brought about by the action of Natural Selection. Weismann has told us that the facts are unintelligible unless interpreted by Natural Selection, and Professor Poulton has told us the same thing. Yet in spite of all this there has grown up in recent years something of the nature of heresy, especially among the younger generation. To some of us the current method of interpreting these remarkable cases of resemblance seems altogether too facile. We are growing a little weary of these reiterated sermons on the unique potency of Natural Selection ; we are beginning to question whether the new facts that keep streaming in do really support that doctrine in the way that its prophets all aver—whether indeed some of them do not indeed constitute serious objections to the explanation with which we are so familiar. In the pages that follow I shall try to put into shape some of these objections. I could have wished that one better acquainted with the material had undertaken the task, someone whose greater familiarity with Lepidoptera would not have laid him open to the charge of ignorance which Professor Poulton has so justly preferred against me. But the great majority of those who

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write upon these matters, at any rate in this country, have already enrolled themselves among the devout, and if the doctrine so fervently preached by Professor Poulton and his acolytes is to be subjected to any criticism it is clear that such criticism must come from a layman. If I have allowed my zeal to get the better of my timidity it is because I have felt that the prevailing doctrine is inadequate, and that if we are to elucidate one of the most fascinating problems in biology the facts must be regarded from a different point of view.

Professor Poulton has stated his position clearly in the last number of this journal, and it is therefore unnecessary for me to do more for the moment than to draw attention to the main point at issue. And that is the part played by Natural Selection in connection with the remarkable resemblances often found among animals which are not closely related. As the phenomena to be discussed are on the whole most striking among Lepidoptera it will be as well to limit ourselves to this group of insects.

We have then in front of us all these cases of remarkable resemblance in colour and pattern between butterflies inhabiting the same region and belonging often to widely different genera and even families—resemblances often so striking that Darwin might well ask why, to the perplexity of naturalists, Nature had condescended to the tricks of the stage.* The problem is to formulate a hypothesis as to how the resemblance has come about. One hypothesis, at present the most widely accepted one, is that associated with the word “mimicry,” on which it is supposed that the resemblance has been brought about through the action of Natural Selection. Species A possesses certain properties which render it unpalatable to some at any rate of its enemies and advertises this fact by a conspicuous pattern or coloration. The more nauseous its taste and the more blatant the advertisement the more its enemies leave it alone, and so the production of this warning coloration in the model is the first function of Natural Selection working to the advantage of the species. Species B also appeals to the enemies of A but has not the advantage of A’s disagreeable flavour, and apparently this unpleasant quality cannot be got into B by Natural Selection operating on the

* *Origin of Species*, 6th ed., 1891, p. 353.

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approved method of benevolent extermination for the good of the victim's more unpleasant relatives.

“ See how the Fates their gifts allot,
For A is happy—B is not,
Yet B is worthy, I dare say,
Of more prosperity than A.”

So Natural Selection once more steps in. For the good of B it must work, for that is its business, and what it cannot accomplish directly it proceeds to bring about by an ingenious fraud. Being unable to make B really unpleasant it does the next best thing by compelling it to appear so. It encourages every slight tendency on the part of B to look like its unpleasant neighbour A, while ruthlessly repressing B's every effort to remain its own pleasant self. And as Natural Selection is one of those things which brook no denial, B is willy-nilly coaxed and pushed into the similitude of A. So “under the guidance of Natural Selection,” to use Professor Poulton's phrase, is the wide gap between the forms of the two species crossed, and B, by being brought to mimic A, comes in for a share of A's prosperity. In short, Professor Poulton's contention is, that B to start with was widely different from A and that the assumption of its disguise has been a gradual process of transition such that if all the stages were before us, they would appear as a long and continuous series showing B insensibly melting as it were into A. The difficulty of accepting such a view is obvious, for it means that we must attribute selection value to the slightest tendency on the part of B to vary in the direction of A's coloration or pattern. We must assume that B's enemies can distinguish perfectly well between A and B in its original form. Is it likely then that they would confuse B with A if B varied only ever so slightly in the direction of A? And unless they do so, how is this slight initial variation to be accumulated through the agency of Natural Selection?

This difficulty was clearly perceived by Darwin, and led him to suggest an alternative hypothesis. The paragraph in which he deals with the matter is instructive and may be quoted in full.

“ It should be observed that the process of imitation probably never commenced between forms widely dissimilar in colour. But starting with species already somewhat like each other, the closest resemblance, if beneficial, could readily be gained by the above means; and if the imitated form was subsequently and

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gradually modified through any agency, the imitating form would be led along the same track, and thus be altered to almost any extent, so that it might ultimately assume an appearance or colouring wholly unlike that of the other members of the family to which it belonged. There is, however, some difficulty on this head, for it is necessary to suppose in some cases that ancient members belonging to several distinct groups, before they had diverged to their present extent, accidentally resembled a member of another and protected group in a sufficient degree to afford some slight protection ; this having given the basis for the subsequent acquisition of the most perfect resemblance.*

We may then infer Darwin's opinion to have been that model and mimic, our two species, A and B, to have been *somewhat like* to begin with. The question now becomes how much alike ? I do not see how we can avoid supposing that they were sufficiently like to have been confounded with one another in the beginning, which is when the enemy first lay in wait for them. Otherwise we are once more beset with the old difficulty of the selective value of slight variations when the selecting agent is a percipient creature.

Let us suppose then that A and B were very like to start with. What we have now to explain is that at the end of the process the mimic B is like A, but unlike, and often very unlike, most of its near relations in other parts of the world. Evidently we must suppose one of two things : either that its relations have moved away from B, or that B and A together have moved away from the semblance of B's relations. We shall probably all agree that the latter of these two suppositions is the more likely. Now if A and B have diverged in unison from their earlier form, it is evident that the cause must be sought in A. For since B is already like A, it must be to its advantage to remain so. We must ask therefore what advantage A is to get by the change, and it would seem that the only answer we can suggest, granting A's unpleasant nature, is better advertisement. For were we to suppose the change in A to be in the direction of protective coloration, we should be stultifying the action of Natural Selection in having gradually produced A's nauseous taste. Or again, if it be suggested that the change advantages A by affording a better recognition mark between the members of that species, it could at once be objected that plenty of butterflies of inconspicuous

* *Origin of Species*, 6th ed., 1891, p. 354.

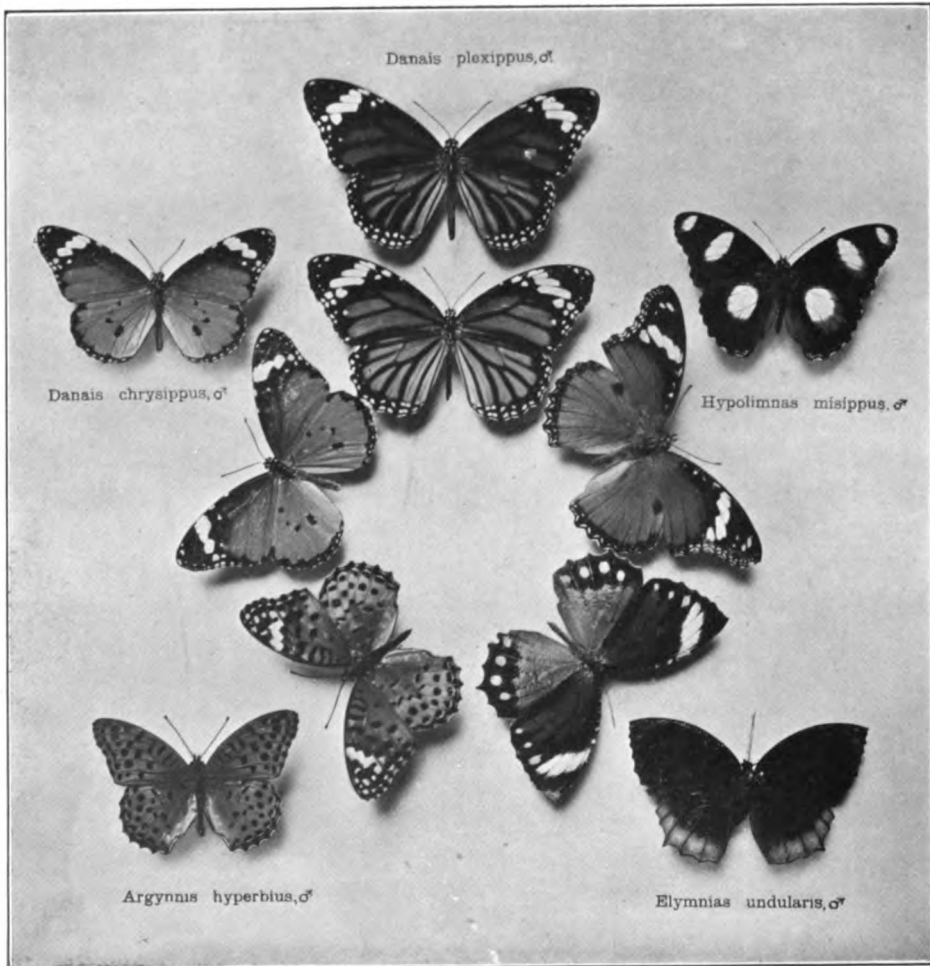
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colouring appear to have no difficulty on this score. We will take it, therefore, that the change from A's original pattern to the new one is brought about through the action of Natural Selection working to A's advantage by favouring any variation towards loudness in its raiment, and thereby building up a warning pattern which shall advertise A's enemies of the fact that it is nauseous. It is true that the difficulty of attributing selective value to minute variations where the selecting agent is a percipient creature is once more met with, but is not so gross here as in the case already considered. For granted that such variations are heritable, and given an enemy with exactly the appropriate discriminating powers, it is conceivable, at any rate on paper, that they might also have selection value. So much for A : now for B.

Since B's advantage consists in preserving its resemblance to A, it is obvious that if A changes through the agency of Natural Selection, the same agency will operate for the advantage of B if it should force it to change in unison with A. Hypothetically this could be brought about by Natural Selection provided that the selecting enemy exercised a discriminating power which was great, but not too great. He must be sufficiently sharp-sighted to notice the difference produced by the alteration in the colour of a few hundred scales on the wing—a difference not readily perceptible to the average human eye—but he must be blind to those general differences of form and texture which are almost always found between model and mimic. Granted, therefore, the accommodating enemy, and granted also the heritability of minute variations, it is conceivable on theoretical grounds that changes in B concomitant with those in A might be brought about through the agency of Natural Selection. Such we take to have been Darwin's view of the manner in which the mimetic resemblance of an unprotected to a protected form has been brought about, and we may now ask whether any facts are available which may help us in coming to a decision as to the possible truth of such a view.

It is a peculiar feature, long familiar, of many of the best known instances of mimicry that the resemblance to the nauseous model is confined to the female sex. In some cases, such as that of *Papilio dardanus* discussed by Professor Poulton, there may be a number of different forms of female, some of which closely resemble different

Plate I.



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models. But there are other cases of a somewhat simpler nature in which, in a given locality, there is only one form of female in the species, and she resembles a given model. As in *Papilio dardanus*, the male is as a rule quite unlike his consort. For some reason or another Natural Selection often fails utterly to bring about mimicry in the male sex, though one would have thought that the species could not fail to profit by the change. But for the moment the manner in which this condition of affairs is brought about need not detain us. What is of interest to the present argument is that the coloration and pattern of the male is supposed to present us with a picture of the condition from which the female diverged when she started on her mimicking career. "In the numberless cases," writes Professor Poulton,* "in which a non-mimetic male is accompanied by a mimetic female, the male bears the ancestral appearance." This is explicit enough, and we may now proceed, with the help of concrete examples, to consider the position into which this assumption necessarily leads us. The examples I have chosen are certain butterflies found on the island of Ceylon, which are illustrated on Plate I. Two of the commonest butterflies on that island are *Danais chrysippus* and *D. plexippus*, and both are characterised by the same general scheme of colour, involving black, white, and a bright yellow-brown. Both are regarded by Professor Poulton as models, owing to their "warning coloration" and also to their presumed nauseous properties. On the whole these two models are much alike and the resemblance between them is even more marked when flying in the open than when set and exhibited side by side in a cabinet. On the island are also three species of butterflies in which the female "mimics" the *chrysippus-plexippus* colour scheme, the male in each case being quite distinct. Two of these, *Hypolimnas misippus*, and *Argynnis hyperbius*, are Nymphalines, while the third, *Elymnias undularis*, is a Satyrine. Now for the argument.

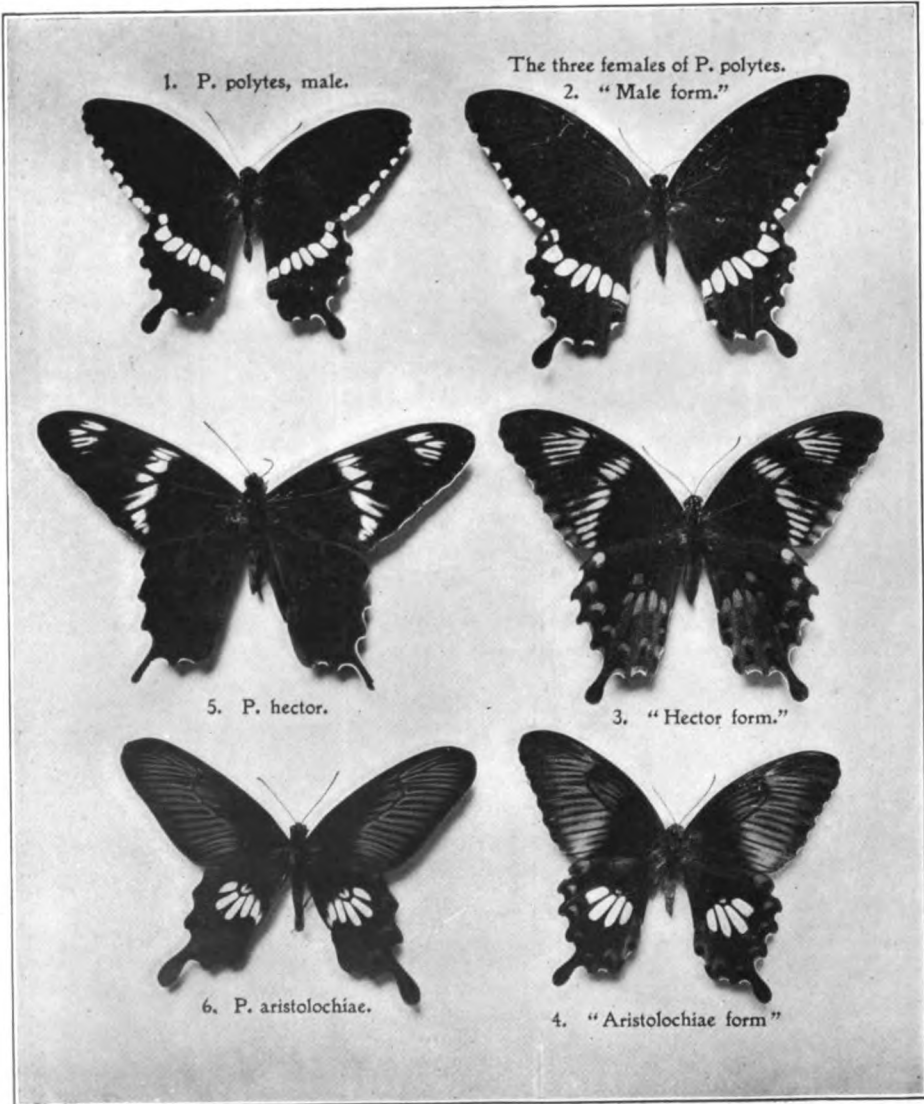
On the mimicry hypothesis the female of *Elymnias undularis*, before starting to resemble *Danais plexippus*, must have been like the purple-brown male. And if in order to get over the difficulty of the selection value of slight variations, we suppose that the *E. undu-*

* *Essays on Evolution*, 1908, p. 244.

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laris female has kept pace with a similar series of changes in *D. plexippus*, we must suppose that the ancestral pattern of this latter species also was at one time similar to that of the male of *E. undularis*. But since the female of *Argynnis hyperbius* also mimics *D. plexippus* the same train of reasoning leads us to the conclusion that the ancestral pattern of *D. plexippus* was at some stage similar to that of the male of *A. hyperbius*. Again, it will probably be granted that *D. plexippus* and *D. chrysippus* have evolved from a common stock in comparatively recent times, and that the series of colour and pattern changes which led to the one form has been almost identical with that which has led to the other. And since the female of *Hypolimnas misippus* mimics *D. chrysippus*, the Danaid must at one time have resembled the male of *H. misippus* with its large white spots edged with purple on an almost black ground. In other words, the *plexippus-chrysippus* combine must in comparatively recent times have passed through stages closely resembling the very diverse males of the three species whose females mimic the characteristic pattern of that combine. Nor is this all, for *D. chrysippus* is mimicked elsewhere, as in Africa, and to the catalogue of ancestral patterns with which our argument endows this species must be added others, such as, for example, that of the male of *Papilio dardanus*, which is utterly unlike any of those yet considered. It is quite clear, then, that we cannot get over this difficulty of the selection value of minute initial variations in the way suggested by Darwin.

We are therefore left face to face with this enormous assumption of Professor Poulton's—that a very slight accidental variation on the part of a species, in the direction of a pattern which is utterly different, will be detected by its enemies and will cause them to let it alone. We may well ask how an enemy endowed with such remarkable powers of discrimination could fail to distinguish between mimic and model even in cases of the closest resemblance yet recorded. For the whole theory of mimicry, as brought about through the cumulative action of Natural Selection on slight variations, depends upon the assumption that the enemy is discriminating but not too discriminating. However, we shall return to the enemy later. But before leaving the subject of slight variations a protest must be entered against Professor Poulton's further assumption



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that any small variation may be inherited. It must be remembered that it is only an assumption, that in no clear case has it been shown to exist, and that it is certainly not countenanced by recent research on heredity.

This difficulty of the initial stages, so clearly recognised by Darwin, and so lightly disposed of by Professor Poulton, is far from being the only serious one attaching to the current theory of the mimicry process. Some further difficulties may, perhaps, be best illustrated with reference to a concrete example. One of the classic cases of mimicry is that of *Papilio polytes* in Ceylon and India. The male of this species is a dark, almost black, fly, with a series of conspicuous creamy-white markings on the hind wing (Plate II., Fig. 1). The females are of three distinct kinds. One is very like the male and we may speak of this as the "male form" (cf. Plate II., Fig. 2). A second bears a resemblance to another common species of *Papilio*, viz., *P. aristolochiæ*, and this we may term the "*aristolochiæ* form" (cf. Plate II., Fig. 3). The third female resembles *Papilio hector* in that white patches are developed on the fore wings, and red markings on the hind ones (cf. Plate II., Fig. 4), and it will be convenient to call this the "*hector* form." The two latter forms were regarded as distinct species until Wallace pointed out that we had here a beautiful case of polymorphism confined to the female sex. Wallace also suggested the explanation based on Bates' hypothesis of mimicry. *P. aristolochiæ* and *P. hector* are *Papilios* which belong to the "Pharmacophagus" group or "Poison-eaters," because their larvæ feed on the well-known *Aristolochia* plants. These two species thereby acquire disagreeable properties and have developed a conspicuous coloration to warn prospective enemies of the fact. The larvæ of *P. polytes* on the other hand feed on citronaceous plants, and there is no reason for supposing that the flavour of the adult is markedly disagreeable. Now on the theory of mimicry certain of the females of this unprotected species developed a touch of white on the fore wing, and a speck of red on the cell of the hind wing. The keen-witted enemy took due note of these minute changes, and having realised that a large patch of white on the fore wings and red markings only on the hind wings meant a nasty taste, thought it best not to interfere with an insect which exhibited even the least trace of these offensive markings, but to confine its attention to

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those individuals in which the white and the red specks were absent from those particular portions of the wings. Then in some way that has never been explained these specks became inherited, continually varied in the direction of greater size, and were worked up by Natural Selection into the condition now found in the "hector form" of *P. polytes*. And in a similar way was evolved the "aristolochiæ form" of female. Apart from the difficulty of initial stages already discussed this case of *P. polytes* offers a further one not hitherto met with in that certain of the females are still like the males. How is it that these can manage to survive under conditions of selection which lead to the elimination of the hypothetical transitional forms? If the postulated series of forms intermediate between the "male form" and the "hector form," which on hypothesis must have had some advantage over the "male form," has been entirely forced out of existence, how is it that the "male form" itself with no advantages still manages to hold its own? For not only does it exist, but, as all observers are agreed, it is in Ceylon considerably more abundant than either of the mimetic forms.

Nor does the peculiar interest of *P. polytes* stop here. Mr. J. C. F. Fryer* has recently succeeded in carrying out an elaborate series of breeding experiments with this species and has shown that any form of female can produce any other form provided that she mates with an appropriate male, while in certain cases all three forms may appear in the same brood. Even in such a case all the three forms are sharply-cut and clear, there being a complete absence of intermediates or transitional forms. In fact as the result of his experiments Mr. Fryer has been able to demonstrate that the inheritance of the various forms of *P. polytes* is in accordance with a simple Mendelian interpretation, and comparable with what we are already familiar with in other forms of life. We know very well that new varieties, often involving considerable alterations in structure, pattern, or colour, may come into being suddenly, without the intervention of transitional forms. The history of cultivated plants teems with such cases. Moreover we know that such variations, complete from the beginning, are transmitted on Mendelian lines,

* Mr. Fryer's results were communicated to the Entomological Congress in Oxford last year. They will probably be published in full during the present year.

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just as are the variations involving the different forms of *P. polytes*. Surely then in the light of existing evidence it is but reasonable to suppose that the variations which characterise the three forms of female in *polytes* have arisen as the result of sudden change or mutation, instead of supposing them to be the product of Natural Selection working on a series of small hypothetical variations, which has never even been shown to exist.

Indeed this case of *Papilio polytes* illustrates very clearly the main point of difference between Professor Poulton and myself, i.e., the conception of the function of Natural Selection with regard to these resemblances. Both of us are agreed as to the reality of Natural Selection. Both of us recognise that Natural Selection cannot *initiate* a new variation. But though for Professor Poulton Natural Selection does not start the variation it works it up from something insignificant to something large and striking. Given the minute initial variation the resemblance of mimic to model is for him entirely the work of Natural Selection. It is here that we part company. I have already argued for the view that sudden variations of appreciable magnitude may arise * and I do not regard it as impossible that at some stage in the history of *P. polytes*, the "aristolochiæ form" may have arisen suddenly from the "male form," or *vice versa*. After all the different females of *polytes* are doing the same sort of thing every day. Were a new form arising in this way to bear a close resemblance to some unpalatable species then I see no difficulty in supposing that it would be favoured by Natural Selection until it became the dominant or perhaps the sole form of the species. But Natural Selection would take no part in the *formation* of the resemblance. Its function would be merely to *conserve* it, and to extend its range after it had already arisen.

But the instructiveness of this case of *Papilio polytes* is not yet exhausted. Professor Poulton has already commented upon the special interest attaching to these peculiar cases where polymorphism has become a character of the species.† "It is, however," he goes on to lay down, "a character that is only kept up by constant selection." Hence we must suppose that a process of constant

* Cf. *Spolia Zeylanica*, Vol. VII., 1910.

† This Journal, Vol. II., No. 1, April, 1913, p. 53.

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selection is going on to the advantage of the mimetic forms and to the disadvantage of the male form. But is this assumption warranted? I do not believe that it is, and for reasons which we may now proceed to discuss. Some years ago G. H. Hardy showed that there were a large number of positions in which a population, whose members differed in respect of a Mendelian character, would maintain itself in equilibrium.* If p be the number of homozygous dominants, $2q$ the number of heterozygotes, and r the number of recessives, then such a population will remain in equilibrium, provided that the values of p , q , and r are such that the equation $pr = q^2$ be satisfied. Moreover, if at a given moment such a population be not in equilibrium, Hardy showed that it will rapidly fall into such a position, and thereafter, other things being equal, will maintain it. It is evident that any population in which the proportions of homozygous dominants, heterozygotes, and recessives, are respectively represented by the three terms of the equation $x^2 + 2xy = y^2$ must fulfil this condition of equilibrium. Hardy's law of equilibrium has recently been expended in an interesting way by Mr. H. J. T. Norton, of Trinity College, Cambridge. Starting with a population in which one of the homozygous forms occurred only very rarely, Norton has worked out theoretically the results which follow from endowing that form with a definite advantage, such as would be afforded by Natural Selection working in its favour. The course of events differs somewhat according as the favoured form is the dominant or the recessive. Where it is the dominant, it establishes itself very rapidly, and soon forms the great bulk of the population; but the last traces of the recessive are only eliminated very slowly. Where the rare favoured form is the recessive, it becomes established relatively very slowly, but towards the end of the process the dominants are eliminated, as it were, with a rush. In either case, however, the process is a comparatively rapid one. Mr. Norton† has calculated that with a 10 per cent. selection advantage (i.e., if the selection were of such stringency that 100 per cent. of the favoured variety survived to

* *Science*, 1908.

† Mr. Norton's results were recently communicated to the Cambridge Mathematical Club, but are as yet unpublished. I am greatly indebted to him for a brief abstract, which I have here made use of.

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lay eggs as against 90 per cent. of the unfavoured), the proportion of dominants would increase from 9 per cent. to 30 per cent. in fourteen generations, from 30 per cent. to 55 per cent. in twelve more, from 55 per cent. to 75 per cent. in another twelve, and from 75 per cent. to 89 per cent. in a further eighteen generations. In other words, if a butterfly population consisted of 9 per cent. dominants (either homozygous or heterozygous) and of 91 per cent. recessives, and if then a 10 per cent. selection advantage were given to the dominants, their proportion would rise from 9 per cent. to 89 per cent. in but fifty-six generations, while during the same time the proportion of the unprotected recessives would fall from 91 per cent. to 11 per cent. And as many tropical butterflies, *e.g.*, *P. polytes*, pass through at least three generations in the year, this remarkable change in the proportions of the mimetic and non-mimetic forms might, even with a selection difference of only 10 per cent., be brought about in less than twenty years. Or, if we look at it in a slightly different way, it would take thirty generations about ten years for the proportion of the non-mimetic recessives to drop from 45 per cent. to 11 per cent. of the total population. For the case of *P. polytes* itself this figure would have to be increased somewhat owing to the males not exhibiting mimicry. Roughly, this would about double the time required. Hence, with a selection value of 10 per cent. in favour of the protected form, the proportion of the recessive "male form" should decrease from 48 per cent. to 11 per cent. in about twenty years. The chief interest attaching to these figures lies in the demonstration of the rapidity with which a mixed Mendelian population must change when even a moderate amount of selection is exercised in favour of one or other variety. We have unfortunately no statistics as to the proportion of the three forms of *polytes* females in the past. But we do know that the "aristolochiæ" female of *P. polytes* has been in existence for over 150 years since it was recorded by Linnaeus. Also the "hector form" was described from Ceylon by Cramer as *Papilio eques Trojanus romulus* in 1776. And the fact that both of the mimetic forms were known before the non-mimetic female certainly suggests that they were not uncommon in the eighteenth century. Why, then, is the non-mimetic the most common of the three forms of female at the present time? For if the mimetic forms were favoured even in a relatively

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slight degree by Natural Selection, the non-mimetic form ought by now to be exceedingly scarce. On general grounds, there would seem to be no escape from the inference that all three forms are on precisely the same footing, and this is supported by the fact that the proportions of the mimetic and the non-mimetic forms, as determined by Mr. Fryer, are just what might be expected on Hardy's law of equilibrium. In a considerable sample, the two mimetic females formed 55 per cent. and the non-mimetic 45 per cent. of the total. We know that the non-mimetic form is recessive, and these figures fit exactly with constitution of a population in which the homozygous dominants, the heterozygotes, and the recessives are in the proportion 1 : 4 : 4—a proportion which satisfies Hardy's law. Moreover, there is no reason for supposing that the relative proportions of these three forms have changed materially for many years. For some time past Ceylon has been, from an entomological point of view, one of the best known of tropical areas, and any marked change in the proportions of the different females of such an abundant and familiar insect as *P. polytes* could hardly have failed to excite comment.* Moreover, Mr. Fryer's researches failed to disclose any constant advantage possessed by the "male form" in some other direction. Were the non-mimetics constantly more fecund for instance, or were their offspring more hardy, these qualities might just be able to compensate an adverse death-rate brought about by Natural Selection, so that equilibrium would be maintained between the different forms. But Mr. Fryer's experiments give us no ground for postulating to the male form any such advantages. Taking all the facts together, then, there would seem to be but one deduction at present to be drawn from them, and that is that *Natural Selection is non-existent in so far as concerns the relation of the mimetic to the non-mimetic females of Papilio polytes.*

It may well be that this will turn out to be true for other mimetic species, especially where, as in *P. polytes*, non-mimetic

* Interesting in this connection is an observation of Wade's, quoted by Moore in his *Lepidoptera of Ceylon*. "These three butterflies are very common, especially those of the first form; the second form being perhaps least so." The first form alluded to is the male form, and the second is the aristolochiæ form. Moore's work was published in 1880, and the condition of things indicated in this statement agrees perfectly well with the idea that no change in the relative proportions of the three forms has taken place in the last 30—40 years.

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individuals exist side by side with mimetic ones. But in the present state of our knowledge it would be rash to say more than that the existence of non-mimetic forms certainly affords a presumption that Natural Selection is not at work in such cases.

With regard to another aspect of Natural Selection in the sphere of mimicry, the question of the efficacy of the direct agent, a few words must suffice. Since the resemblance of mimic to model is in many cases confined to the upper surface of the wings, we must suppose that the resemblance can only be of use when the upper surface is exposed, i.e., when the insect is flying. If, therefore, there is any selection, it must be effected by an agent which attacks butterflies on the wing. Butterflies are preyed on by birds and by predaceous insects.* Asilid flies are the cause of considerable destruction, and where they occur they are perhaps the most dangerous enemies that butterflies have; but the evidence is all against their exercising any discriminating powers. Neither is there any good evidence for ascribing such powers to wasps or dragon flies. We may therefore leave predaceous insects out of account as factors in the production of mimetic resemblances, and confine ourselves to birds. On this subject much has been written, for it has been more or less clearly felt by the supporters of the mimicry hypothesis that unless birds are the selecting agents there is nothing else that can bring about the postulated changes in the postulated way. I do not propose to enter into the scattered literature relating to the attacks of birds on butterflies. Much of it has recently been brought together by Mr. G. A. K. Marshall in a careful paper to which the interested reader may be referred.† There is no doubt that birds do attack butterflies, though it is doubtful whether their ravages approach the severity that some would have us believe. Mr. W. Schaus, for instance, with an unrivalled experience of the forests of tropical America, where "mimetic" forms abound, has come to the conclusion that destruction by birds is almost non-existent.‡ But

* Butterflies are also preyed on by lizards, and to some extent by monkeys. Lizards are apparently quite indifferent to the distinction between "nauseous" and "palatable" forms, so that there are no grounds for supposing them to have anything to do with mimicry. With regard to monkeys, observations are too scanty to come to any conclusion.

† *Trans. Ent. Soc.*, 1909.

‡ I^{er} Congrès Internat. d'Entomol., Bruxelles, 1911.

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even if the postulated destruction be granted this is not enough. If birds are to bring about the required changes their attacks must be discriminating ; and upon their powers of discrimination the whole hypothesis of mimicry rests. Unfortunately we know very little indeed about the tastes of birds under natural conditions. Many of the records are of attacks upon presumably unpalatable models, and a single clear and convincing case where the evidence of fact is in favour of a mimic deriving advantage from its resemblance to a model is yet to be worked out. On the other hand, there is some evidence that the reverse may be true. A pretty instance of this has just been put on record by Mr. Fryer. Among the most abundant butterflies in Ceylon are the black and light blue Danaiids, and the dark brown Euploeas. Both of these groups are regarded by Professor Poulton as among the most distasteful of butterflies, and in Ceylon they are cited by him as models for *Papilio clytia*, *Hypolimnas bolina*, *Nepheronia ceylonica* (a Pierid) and other mimicking species. Indeed in the language of mimicry they are among the most predominant models of the Indo-Malayan region. From observations extending over nearly two years, Mr. Fryer agrees with Colonel Manders and others that birds in general are not formidable enemies to butterflies in Ceylon—with, however, one exception. The Wood-Swallow (*Artamus fuscus*) lives almost entirely upon butterflies. Moreover it exercises discrimination in the species which it attacks. But unfortunately for the mimicry hypothesis it is the distasteful models, the Danaiids and the Euploeas upon which it lives almost to the exclusion of other forms. As Mr. Fryer points out, any resemblance to these so-called models on the part of other species must be regarded as a source of danger to them rather than as a safeguard. But of course much stress cannot be placed on this apparent discrimination on the part of the Wood-Swallow. Probably it does not mind what butterflies it eats. “Alles ist Wurst,” but the slow-flying Danaiids and Euploeas are much easier to catch. The attitude of the Wood-Swallow seems clear enough, and it has yet to be proved that other insectivorous birds take a different view of butterflies as articles of diet. Until such evidence is forthcoming it is impossible to regard “mimicry” and all that it implies as to the mode of operation of natural selection as anything more than an ingenious hypothesis which is fast

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crumbling to pieces under the disintegrating influence of fresh facts.

It is an hypothesis which, in its present form as advocated by Professor Poulton and others, confers upon minute variations a selective value which is inconceivable when regard is had to the nature of the selecting agent ; it makes the sweeping assumption that such minute variations are inherited : it is driven to argue for an utterly unknown and mysterious process by which these minute variations can be built up into a widely different and fixed form : it is unable to account for the absence of transitional forms when the germ-plasms of the old form and the new one are mixed : it has no adequate explanation to offer for the frequent absence of mimicry in the male sex : it leaves without any solution those numbers of cases of polymorphism where there is no question of mimicry : and lastly it endows birds with powers of selective destruction which are certainly not deducible from the available evidence. Surely it is time for the whole business to be considered in a more critical spirit than has hitherto been the fashion.

In conclusion, a few words with regard to the argument against abrupt changes of pattern which Professor Poulton gives in the last number of *BEDROCK*. I gather that the gist of it is briefly as follows : *Papilio dardanus* is a species exhibiting an extraordinary amount of polymorphism among its females. Some of them closely resemble various Danaine models—others, however, do not. Of these non-mimetic females some in colour and pattern are more like the male than are the mimetic females. These are the postulated transitional forms. Therefore there has been a gradual change from females closely resembling the male to the markedly distinct mimetic forms.

Let us, for the sake of argument, leave out of account the fact that some at any rate of these transitional forms (such as *trimeni*) are specific, and suppose for the moment that the various forms of female can be arranged to suit the fancy of those who wish for a complete grading series. Does it, therefore, follow that the various forms have arisen through the accumulation of minute variations ? Certainly not. We are acquainted to-day with apparently continuous series where experimental breeding has shown that inheritance is, nevertheless, on strictly Mendelian lines. The Silky fowl

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is a case in point. This breed is characterised by the abundance of black pigment in the deeper layers of the skin and in various tissues. Owing to this peculiarity it is sometimes known as the negro fowl. When the deeply pigmented Silky hen is crossed with the Brown Leghorn cock the offspring show a small amount of the peculiar pigment. These when bred together give a long series ranging from the deep pigmentation of the pure Silky to its complete absence as in the Brown Leghorn. By judicious selection a fairly continuous series of intergrading forms between the two extremes can be put together. Nevertheless, this series can be expressed in terms of two Mendelian factors—a factor for pigmentation and a factor which more or less inhibits the action of the pigmentation factor. The apparently continuous series is due to the fact that the birds may contain either two, one or no doses of the pigmentation factor, combined with either two, one or no doses of the inhibitor factor. Thus an appearance of continuity in variation may be brought about by the interaction of a small number of definite factors upon one another, and the mere fact of the existence of a series of apparently transitional forms is no longer a convincing argument for supposing the connection between those forms to be of the nature advocated by Professor Poulton. It will be time enough to discuss the relation of these so-called transitional forms to one another and to the mimics when we know something of their genetics. Nor must it be forgotten that the production of these transitional forms may be to some extent dependent on the conditions of temperature and moisture during the later larval and the pupal stages. Indeed, Professor Poulton quotes an experiment of Mr. Lamborn's on *Papilio dardanus*, suggesting that this may occur, and some experiments of Colonel Manders with *Hypolimnas misippus* and *Danaüs chrysippus* also point in the same direction (cf. *Trans. Ent. Soc.*, 1912). But here again the matter is rather one for further experiment than dogmatic assertion.

And here I may add a few words on another point with regard to which I have been taken to task by Professor Poulton. The two forms *Hypolimnas (Euralia) wahlbergi*, i.e., *wahlbergi* and *mima*, mimic respectively *Amauris dominicanus* and *A. echeria*. Breeding experiments have shown that the relation between *wahlbergi* and *mima* is a simple Mendelian one—that the difference between these

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two patterns behaves as though it were due to the operation of a single factor. I suggested that with regard to the parallel patterns of the two *Amauris* models we are concerned with a difference which also can probably be expressed in terms of a single factor. Professor Poulton calls this an "extraordinary statement" and seems unable to realise how, "although model and mimic are in every other respect widely different, in this one single but highly complex feature of pattern they are identical." But why not? Stamp a pattern on a piece of velvet and again upon a piece of cretonne—the effect is different but the pattern is nevertheless identical. Or better still, take a Brown Leghorn cock with its characteristic coloration and markings and replace the normal structure of the plumage by the Silky type of feathering. The sharp hard outlines between the various colours in the pattern are at once softened and the whole colour tone is changed. Nevertheless the pattern is identical in the two cases and the difference, marked though it is, depends upon the presence or absence of a single genetic factor connected with the feather structure. But Professor Poulton will have none of it because Aurivillius some fifteen years ago placed *dominicanus* as the second and *echeria* as the fifteenth species of *Amauris*. Moses put the weasel first among forbidden creeping things, while the ferret was placed fourth. But we do not therefore argue to-day that these two creatures are less closely related to one another than either is to the tortoise which comes between them on that ancient list. Modern research has suggested that a character in species A may well be identical with a character in species B, though in many other characters the two species may be wide apart. There is every reason for supposing that the chocolate and also the blue coat-colours in mice and in rabbits depend for their manifestation on identical pigmentation factors. Yet in other features the two species are widely separated. To anyone familiar with modern work on heredity the suggestion that the difference between patterns of *Amauris dominicanus* and *A. echeria* is the outcome of a single factor offers no inherent difficulty. And when it is known that two similar patterns in another genus depend upon the presence or absence of a single factor, the suggestion for *Amauris* receives very strong support. The same two colour-patterns occur also in the *cenea* and the *hippocoon* forms of *Papilio dardanus*. When the

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genetics of this species is worked out it will probably be found that here also the difference is one of a single factor. Indeed, the genetics of these polymorphic forms offers a fascinating field of research from the phylogenetic point of view. When the various species in which there is overlapping of similar patterns are properly worked out genetically we shall probably be able to obtain some insight into the course of evolution with regard to these colour-patterns. *Papilio dardanus*, for instance, will give us an opportunity of determining the relation between the *trophonius* form and the *cenea* and *hippocoon* forms, and so of indirectly relating the Danaines *D. chrysippus*, *A. echeria* and *A. dominicanus*, where direct crossing is probably out of the question. Such work certainly offers a fairer hope of establishing phylogenetic relationships than does trusting blindly to the order of arrangement which some one else found convenient to adopt, or wildly assuming that because a form lives on an island it is therefore ancestral. In any case we cannot accept these two arguments of Professor Poulton, viz., the existence of rare transitional forms and the denial that identical genetic factors may exist in different species, as necessarily implying the process of gradual and continuous change for which he pleads. Whether evolution has ever taken place in this way or not we do not yet know enough to say for certain. But we can say that there is at present no good evidence for supposing it to have done so, whereas there is strong evidence for supposing evolution to have proceeded by definite mutational stages. For these last are the only variations which we know surely to be inherited, and unless variations are inherited they can play no part in evolutionary change.

PRE-PALÆOLITHIC MAN

By J. Reid Moir, F.G.S.

IN this paper I intend to discuss the evidence furnished by various chipped flints recently discovered, as to whether man was present on this earth in ages long prior to those in which the ordinary leaf-shaped and ovate palæolithic implements of the river gravels were made, and to compare the present day opposition to these recent discoveries with that which was at one time brought against the now universally accepted palæolithic implements.

There is no doubt that the difficulty of determining whether a flaked flint owes its form to human or natural agencies, has always been a very pressing one with students of pre-history, and although it appears that no discussions were ever held as to the "humanity" or otherwise of the neolithic implements, yet we know that even the symmetrical and beautifully formed axes and arrow-heads of this period were once considered to have had a supernatural origin and were described respectively as "Thunderbolts," and "Elf-arrows." *

The discussions, however, which were held regarding the human origin of the palæolithic implements discovered by M. Boucher de Perthes in the valley gravels of the River Somme in France about the year 1841, are fortunately fully recorded, and can be read by anyone who wishes to do so.†

In the present state of our knowledge of pre-history it seems most strange that such obvious works of man as the palæolithic implements should have been so strenuously denied, and remain unaccepted by the majority of archaeologists for twenty-five years after their discovery, and it is also difficult to see what good purpose such opposition served.

* Evans, Sir John, *Ancient Stone Implements of Great Britain*, 2nd ed., pp. 56 and 362.

† Callard, Pattison, etc., *Antiquity of Man*, 1880.

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But possibly it had the effect of stimulating further researches, and in consequence of finally establishing the "humanity" of the river gravel implements beyond any doubt or cavil.

The most ancient of these palæolithic implements, known as the Chelles type,* are those found in the oldest gravels of our river-valleys, and are looked upon by many pre-historians as representing the earliest efforts of man in flint flaking.

Thus the discovery of various kinds of flaked flints in deposits undoubtedly much more ancient than those containing the Chelles implements, and which are looked upon by many, including myself, as undoubted works of man, has initiated another period of unrest and disagreement in prehistoric archæology.

Though I propose to deal principally with those discoveries with which I personally have been associated, I do so only because I feel able to speak with some degree of certainty about them, and I have no wish to minimise in any way the splendid work done many years before I commenced my researches by men like Mr. Benjamin Harrison, of Ightham in Kent, and other investigators, both in this country, and on the continent of Europe.

The plateau of Suffolk, through which our present rivers have eroded their channels, is composed near Ipswich of the following beds. I give them in descending order :—

Chalky Boulder Clay,
Middle Glacial Sand and Gravel,
Red Crag,
London Clay,
Woolwich and Reading and Thanet Beds,
White Chalk,

and it is in and under the first three of these deposits that I have found the flint implements dealt with in this paper.

The Chalky Boulder Clay and Middle Glacial Gravel belong to the period known as the Pleistocene, while the Red Crag is generally referred to the preceding epoch—the Pliocene.

An examination of the accompanying diagram will at once show the relation of these implementiferous plateau deposits to those

* This designation has been given to these particular implements owing to their having been first discovered at Chelles-sur-Marne, a place about eight miles east of Paris.

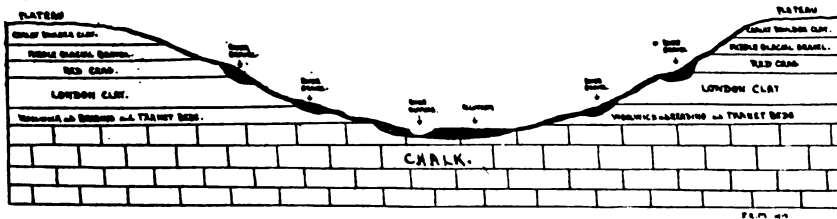
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within the river-valley itself, and laid down during its excavation, which contain the ordinary Chelles and St. Acheul palæolithic implements.

The various plateau beds shown in the diagram at one time extended continuously across the space now occupied by the river-valley.

Before proceeding to describe the flaked flints which have been found beneath the Pliocene Red Crag, and in the early Pleistocene Middle Glacial Gravel, and Chalky Boulder Clay, I would like to draw attention to the palæolithic implements of the Chelles type which, as I have said, are supposed by some to afford the earliest authentic evidence of man's presence on the earth.

These specimens are either of the pointed form, or roughly



Diagrammatic Section across the river Gipping, near Ipswich, showing the relation of the implementiferous plateau beds dealt with in this paper to the river-terrace gravels containing the Chelles and St. Acheul palæolithic implements.

oval, and exhibit a knowledge of flint flaking on the part of their makers, of such an order as to make it impossible to believe that such a proficiency was attained, without long periods of apprenticeship preceding it, when less advanced implements were made. I think I am not overstating the case when I say that no really unbiased person examining a series of typical Chelles implements, and remembering that no particular stage of culture has ever been arrived at, at one leap, could do other than agree that these flaked flints mark but a stage in the slow process of cultural evolution, and therefore more rudimentary types must have preceded them.

Thus those of us who hold this view have proceeded to examine those deposits anterior in age to the beds containing the Chelles implements, and have found as we expected, a series of flaked flints leading up from the most primitive and simple types to others

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more advanced, which are prophetic of those of the succeeding Chelles culture.

In giving a short description of these various pre-palæolithic specimens, I will first deal with those which are found lying upon the surface of the London Clay and under the Pliocene Red Crag.

The top of the London Clay was at one time a land surface inhabited by man, this land surface being afterwards slowly submerged and covered by the sands and shells of the Red Crag Sea.

The Sub-Red Crag implements * are distinguished by the boldness of their flaking and the general massiveness of their form, and the excavations which have been conducted during the last three and a half years have now brought to light a complete "industry" from below this Pliocene deposit.

That is to say a series of flaked flints have been discovered which can be classified into groups comprising choppers, scrapers, pointed weapons for use in the hand, rubbers for dressing skins, hammer-stones and flakes—in fact most of the usual implements (only differing in form and make) found in deposits of palæolithic and later date.

It is also of interest to note, that Mr. W. G. Clarke, of Norwich, has found similar humanly flaked flints below the base of the Norwich Crag.†

The Norwich Crag is almost certainly later in date than that of Suffolk, and the implements from the two areas, though of the same order, exhibit a difference in their forms which is easily observable when a good series from each locality is examined.

The Middle Glacial Gravel, which in Suffolk overlies the Red Crag, has next to be considered. This Glacial deposit is supposed to be intermediate in age between the Contorted Drift of Cromer, and the Chalky Boulder Clay ‡ and from the flint implements of

* Lankester, Sir Ray, "On the discovery of a novel type of flint implements below the base of the Red Crag of Suffolk proving the existence of skilled workers of flint in the Pliocene Age," *Phil. Trans.*, Series B., Vol. 202, pp. 283—336. Moir, J. Reid, "The Flint Implements of Sub-Crag Man," *Proc. Prehistoric Soc. of East Anglia*, Vol. I., part 1, pp. 17—43.

† Clarke, W. G., "Implements of Sub-Crag Man in Norfolk," *Proc. Prehistoric Soc. of East Anglia*, Vol. I., part 2, pp. 160—8.

‡ Harmer, F. W., "The Glacial deposits of Norfolk and Suffolk," *Trans. Norfolk and Norwich Naturalists' Soc.*, Vol. IX.

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apparently varied ages which it contains I would infer it is in part composed of a land surface possibly broken up and redeposited by water, resulting from the melting of an ice-sheet.

The Middle Glacial Implements * differ very markedly from those found below the Red Crag, being smaller and fashioned by more delicate blows.

I recognise at least four different types of implements in this gravel, which by their form, flaking, and mineral condition I look upon as having been made at different, and perhaps widely separated periods, prior to the deposition of the bed in which they are now found.

The oldest-looking series approximates very closely to the specimens discovered by Mr. Benjamin Harrison in the plateau "drift" of Kent, while those I consider of later date show an increasing proficiency in the art of flint flaking.

Overlying the Middle Glacial Gravel is the Chalky Boulder Clay, a deposit laid down during the last great extension of the ice of the glacial period.

In this clay I find another series of implements † which are altogether different from any of those already described, the majority being quite unweathered and unabraded, and some approaching in form the earliest Chelles [palæolithic] specimens.

A reference to the diagram on p. 167 will show that after the deposition of the Chalky Boulder Clay, the river-valley began to be developed, and as it is in the earliest of the deposits of the river that the Chelles implements occur, it will be seen that the Chelles-like character of some of the Boulder Clay specimens is quite in accord with the view of a cultural evolution in flint implements.

This completes my description of the pre-palæolithic specimens of Suffolk, but before proceeding to discuss the objections which have been raised against their human origin I would like to draw special attention to a most peculiar and definite form which has been found to occur in all of the three plateau deposits which have been described.

* Moir, J. Reid, "Flint Implements of Man from the Middle Glacial Gravel and Chalky Boulder Clay of Suffolk," *Man*, March, 1913, pp. 36—7.

† *Ibid.*

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I refer to those which Sir Ray Lankester has designated as being of the "Rostro-carinate," or "Eagle's beak" type.*

These rostro-carinate specimens have all been made upon a definite plan † which consists in the formation of a flat ventral surface, a keel or carina, and a somewhat overhanging point, the point of the "beak."

They occur in the detritus-bed below the Red Crag and exhibit the large surfaces of fracture typical of that horizon.

In the Middle Glacial Gravel, though the general form is the same, the flakes removed are smaller and there is a more finished appearance about the specimens.

The point of the beak has often been "undercut" by most skilful flaking, giving the specimen an even closer resemblance to the beak of an accipitrine bird than is seen in the Sub-Crag specimens.

When the Chalky Boulder implements are examined it is seen that the rostro-carinate specimens are very rare and badly formed, and are evidently being supplanted by the Chelles-like implements already mentioned.

Thus in these three deposits we see the gradual evolution, consummation and disappearance of a complex type of flint implement, and we have also seen that during the period between the laying down of the Sub-Red Crag detritus-bed and the Chalky Boulder Clay flints were undoubtedly flaked in at least six different ways.

These are the facts of the case, facts which can be partly verified by an examination of the case of rostro-carinate implements exhibited in the department of British and Mediæval Antiquities at the British Museum, and completely so by the series of Sub-Crag, Middle Glacial, and Boulder Clay implements exhibited in a special case in the Museum at Ipswich.

Our explanation of these facts is that these various flints have been flaked by man to suit his needs at various epochs in the past, and further we state that an examination of these specimens shows

* Lankester, Sir Ray, "On the discovery of a novel type of flint implements below the base of the Red Crag of Suffolk proving the existence of skilled workers of flint in the Pliocene Age," *Phil. Trans.*, Series B., Vol. 202, pp. 283—336.

† Moir, J. Reid, "The Making of a Rostro-carinate Flint Implement," *Nature*, Nov. 21st, 1912, p. 334.

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a gradual improvement in form and flaking, during the periods in which they were made.

We also support our view as to their human origin by demonstrating that by the ordinary methods of flint flaking, that is by striking one stone with another used as a hammer, we can produce very similar forms.

Now what is our opponents' case? In the first place the geological age of the beds in which the flints were found was disputed, but as time went on and the various pits were visited by expert and well-known geologists, it was seen that there could be no doubt as to the antiquity of the deposits and the contained specimens, and so this initial difficulty finally disappeared.

Then it was said that the rostro-carinate flints could not have been put to any useful purpose and would be useless as implements, and that it was impossible to believe that one type of implement would remain unchanged over such a prolonged period as is represented by that intervening between the Sub-Crag detritus-bed and the Chalky Boulder Clay.

These are ingenious arguments and at first sight rather formidable, but when it is remembered that we do not yet know to what "useful purpose" the palæolithic implements were put, and that the rostro-carinate flints show a *marked* difference in make during the period mentioned, and further that the ordinary round-ended scraper has remained unaltered in form since the most remote times, and is still used by the present day Esquimaux, these arguments are seen to be valueless.

It has also been said (1) that such forms as we collect can be picked up in cart-ruts where they have been formed by the pressure of the passing wheels.

(2) That we have made a selection of certain specimens from a mass of naturally broken flints, and that in such a mass there is every gradation from a flint which has without doubt been flaked by Nature, up to those which are more extensively chipped and which we look upon as of human origin.

Neither of these objections carries any real weight, as cart wheels were not known in pre-palæolithic times, and further no specimens produced by such pressure have "been laid upon the table" for examination.

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It is also an exaggeration to say that "masses" of broken flints occur in the deposits from which the implements are derived.

A certain number of broken flints occur in every gravel. Many of these have undoubtedly been fractured naturally; others, however, certainly have not.

To any one who has examined a "workshop floor" of Palæolithic and Neolithic age, it is clear that a very large number of pieces of flint of meaningless shapes are produced in the manufacture of finished implements.

This must of necessity be so, and when flaking flints myself, I find that such odd fragments are continually being made. In the same manner a carpenter when making a chair or other piece of furniture leaves various pieces of wood about which, regarded alone, would appear to have no relation to the finished article.

So it is with flint implements, and as flint is practically indestructible it is not to be wondered at that we meet with many of these "rough outs" in implementiferous gravels and other deposits.

But when these outlying defences are destroyed our opponents fall back upon the ancient fortress of "Natural Forces" and there take up their last stand against the adoption of a rational view of the antiquity of man.

The argument is put forward that we do not know what these mighty forces of Nature are and what they can do to flints—and further that we never shall.

I, on the contrary, say we know, or ought to know, very well what are the natural forces which can fracture flints, and that they are, thermal action, percussion, and pressure.

The first we need not consider as all are agreed as to the nature of a thermal break upon a flint, the other two forces I have experimented with extensively* and so far have signally failed to produce any forms of flaked flints which really resemble the specimens we look upon as being of human manufacture.

I have, however, been able to ascertain various facts which tend to make me fairly confident as to what is a humanly struck flint and what is not.

* Moir, J. Reid, "The Natural Fracture of Flint and its bearing upon Rudimentary Flint Implements," *Proc. Prehistoric Soc. of East Anglia*, Vol. I., part 2, pp. 171 and 184.

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If our opponents said that their knowledge of this subject was such as to prevent them saying anything definite about it, I should consider it strange, but would realise that this was at any rate a *reasonable* attitude of mind.

But when they state that natural forces have without doubt flaked the flints under discussion, and yet are quite unable to name the exact force responsible, I unhesitatingly say that, in my opinion, such an attitude is unscientific and unsound.

When it is realised that these pre-palæolithic implements of Suffolk can be divided up into six different types, each flaked in a peculiar manner, and that we are asked to believe that some unknown, non-human force has produced these varying forms, we can perhaps form some faint idea of the lamentably sorry state to which our opponents are reduced.

I cannot but feel that something other than single-minded scientific interest animates these gentlemen, and have often wondered whether there is a residue of the old theological opposition to the Antiquity of Man still left in their minds.

This was one of the chief causes which gave rise to the almost fanatical opposition meted out to the first-discovered palæolithic implements, and I cannot do better than complete my paper by quoting from the preface of a book entitled *The Antiquity of Man* (pp. 31—5), consisting of papers, selected chiefly from the *Transactions of the Victoria Institute*, and which preface undoubtedly represents the considered views of those who were opposed to the “humanity” of the palæolithic flints.

I would ask the reader to compare the following objections to these specimens with those which I have mentioned as being brought against the pre-palæolithic implements recently discovered.

“*Origin of Palæolithic Implements.*—But we would ask the further question, are these chipped flints of the Somme, and those of Kent’s Hole, beyond doubt of human origin ?

“After a very careful examination of some hundreds of specimens, the conclusion which we reach is, that man never touched them until they came into the possession of the geologist or the modern workman by whom they were exhumed.

“It must be admitted that there is sufficient *resemblance* between these chipped flints and those of the savage of modern times just to suggest the *question* whether or not man had chipped

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the former, as he has *undoubtedly* done the latter ; yet, when all the circumstances connected with them are considered, the weight of evidence is vastly against the chipping being the work of man.

“ *Palæolithic Implements lacking evidence of Design.*—In the first place these flints of Amiens and St. Aucheul, if chipped by design, exhibit a dexterity which very few civilised men have been able to copy, and yet are lacking the simplest contrivance to make them available as weapons.

“ *Spear heads and arrow heads* are comparatively harmless things ; it is only as spear heads and arrow heads imply spears and arrows, that they convey the idea of weapons ; but here these implements signally fail : this is the very point on which they differ from the spear head or arrow head of the modern savage, or of Neolithic man ; for these ancient flints are not so formed as to enable them to be attached to shaft or reed, having neither notch nor tang, and are often, on the reverse side left unchipped just where the chipping is most needed. Besides which the largest seldom exceeds six or seven inches in length, and that without a shaft, would be but a sorry weapon with which to do battle with the mammoth, the cave-hyaena, or *ursus major*.

“ Then, secondly, no other work of man is found along with them.

“ In certain breccia in Dordogne, bone needles have been found, and rude carving, but they belong to a more recent period.

“ There are no needles or carving found with the implements of Palæolithic man ; chipped flints by thousands, but nothing else ; and it is very difficult to think of a race of men doing nothing else than chipping flints, generation after generation, and that for thousands upon thousands of years as the advocates for the antiquity of man suppose.

“ And observe, the race is making no progress. . . .

“ Wherever palæolithic implements are found, whether in France or England, their defects are the same. . . .

“ Then fourthly, these chipped flints, whether in England or France, are always met with just where you might expect to find them if the chipping was the result of accidental concussion.

“ They are found in the coarse gravel drift, not in the vegetable soil, which might have been the hunting ground or the battle field.

“ These would have been the places to have looked for man’s weapons and implements ; but there you look in vain.

“ We have said that these palæolithic implements have a *certain* resemblance to the weapons of the modern savage ; but instead of drawing the inference that because man made the latter, therefore man made the former, the resemblance we attribute to a natural cleavage in the flint which gives to it a tendency, however struck or crushed, to break into these particular forms.

“ Putting this thought to experiment, the writer has spent some hours in roughly breaking flints with a sledge-hammer, and the result has been that he has found among the broken flint, forms

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sufficiently *resembling* the supposed arrowheads, spear-heads, serrated edged saws, etc., to convince him that as many years spent in this way as M. Boucher de Parthes and Dr. Rigollot have occupied in their research at Abbeville and St. Acheul, would be likely to result in finding amongst the broken flints the choice specimens that they have treasured up ; for it must be borne in mind that they simply made a selection ; it was not every broken flint that looked like a spear head or hatchet, and it will never be presumed that all the broken flints were broken by the hand of man ; therefore, whether these flints were crushed by glaciers, or in any other way unknown to us, the mere breaking of flint is not a difficulty that has to be met.

“ As then these flints of the Somme Gravels are not associated with human remains, and as the vastly greater number have been produced by nature, the probability, to our mind, lies on the side of nature having produced them all.”

In harmony with Principal Dawson's observations are those of Mr. Whitley, who says, “ There is a gradation in form from the very roughly fractured flint, so rude that it cannot be ascribed to human workmanship, up to the most perfectly formed flake of the arrow-head type. . . .”

Mr. Whitley tells us that he has gathered from a heap of flint, undesignedly broken for the repair of the road, at Menchecourt, most perfect flint flake knives, and long, thin, delicately formed “ arrow-heads ” of the most convincing forms.

“ And we suspect that all localities where chalk and flint are found yield specimens of this kind.”

It will be seen that the mental attitude of the opponents of early man has changed very little during the last thirty years, and in fact I do not think that any of our present day opponents has ever put his case forward so cogently and well as was done by the anonymous writer of the preface quoted above.

But what is the position to-day ?

The palæolithic implements are universally accepted, and those who so vehemently opposed their “ humanity ” are looked upon by all archaeologists with wondering amusement.

It is with such a feeling, I submit, that our descendants will look upon those who at the present day and in the same manner are trying to put back the clock of time by attempting to limit man's antiquity to the period when the first palæolithic implements were made.

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There is one thing and one thing only left for the opponents of the pre-palæolithic implements to do if they wish their views to be taken seriously, and that is to subject flints to some unguided, natural force, and produce forms indistinguishable from those which are in dispute.

Even if they can do this their case will not be finally *established*, but it will at least show that they have attempted to support their theories by definite and demonstrable experiments.

SCIENTIFIC MATERIALISM

By Hugh S. Elliot

DR. McDougall, in his article in the last number of *BEDROCK*, has applied the name "materialism" to the views which I had previously published, and which I have the honour of sharing with Sir Edward Schäfer, Professor Loeb, and many others not mentioned by Dr. McDougall. Historically and scientifically he is entirely justified in the use of this name: the views which I represent are the modern equivalents of the earlier materialistic philosophies. But it is none the less a fact that the name "materialism" carries with it many connotations in the public mind, which are very far from the opinions of those who profess it. It is on this ground that "modern materialists" have been very backward in accepting the appellation of materialism. Their beliefs are indeed wholly different from those of Democritus, Hobbes, or other true materialists. The modern lines of scientific thought are more recent than the philosophic terminology in which their results have to be expressed. There is no unexceptionable name to represent them: and, under ordinary linguistic rules, the new philosophy comes to be called by the name of the old philosophy, which on the whole it most resembles. The distinction seems best to be met by using the name "scientific materialism."

Whereas there is some confusion as to names, there need be none as to the things themselves. Materialism, in the new sense, has lain at the basis of science for centuries past. Copernicus founded astronomy on it, and Bacon set it up as the basis of scientific method. It has generally been unpopular, but it has been successful and has spread gradually from one science to another until all are now animated by it. In the nineteenth century it stood out in the

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"science v. religion" controversy. The evolution theory stood for materialism : the evolutionists were the philosophic representatives of materialism at that epoch. And now, in physiology, it is the mechanists who stand once more for materialism : it is the vitalists who represent the popular view. On the one side are the scalpel and the microscope : on the other, "intuition" and ghosts. If we use the word materialism, then, let us recognise what it means. To our opponents it gives the advantage of calling up popular hostility. To us, however, it gives the advantage of association with a winning cause, and with the methods introduced by the greatest men of the past : the men who built up science and shattered superstition, notwithstanding the obloquy showered upon them.

The popular attitude is fortunate in finding a scientific representative in Dr. McDougall. For in general the popular view is inarticulate. Spiritualism, with its various derivatives, is rarely supported by argument : sentiment, not logic, is its foundation. Some vitalists, such as Dr. Hans Driesch, do indeed attempt to meet argument by argument : but his method is a parody of logic that is almost laughable. For he bases his conclusion upon the method *per exclusionem*, which I believe no logician, either in his or our country, would venture to defend as valid in any biological discussion : and even supposing it to be valid, he omits to submit the facts to the preliminary examination required before the method can be brought into use. The naked forms of logic are scandalously prostituted in public : and this outrageous assault upon an ancient and respectable science meets (I am told) with the approval of some among the spiritualists.

Sentiment, then, is what we scientific materialists are up against. Among the masses, that sentiment arises from the slobbering inefficiency which cannot face the hard reality of truth, but prefers to dwell in a heavily-scented and unwholesome atmosphere of lies. Among the classes (I do not use the word in the sense of popular snobbery) there is an attempt to defend spiritualism on more intellectual lines. But I contend that even a physiological defence of vitalism derives its force for most people from remnants of spiritualistic sentiment by which they are affected. The combat is too unequal : on the one side, a mere abstract desire for academic truth ; on the other side, all those powerful emotions from which

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spring spiritualistic and religious beliefs; emotions which for centuries, nay for millenaries, have regulated the very structure of society, and which, though now decaying in their ancient form of sectarian dogma, leave an aftermath of spiritual craving that crops up in every vacant spot.

Even Dr. McDougall does not seem to be altogether free from the promptings of a refined sentiment. In the preface to his *Body and Mind* he alleges the enormous importance for social morality and welfare, of a continued belief in animism and a future life : and he states his desire on that ground to see these views substantiated. Further, he objects to my description of certain spiritualistic views as a "degrading type of materialism." This, he says, is "a matter for the editors," not for him ; and he thus indicates an emotional bias of such strength as would deny freedom of speech to one who should attack it. For the adjective in question is one which has been applied commonly enough and with entire public approval to the views which I hold : and it is an adjective, therefore, which I claim the right to apply to the superstitions held by the general public.

In general, Dr. McDougall's courteous article is liable to the criticism which is applicable to almost every attack on the materialistic position : namely, that of ignoring altogether many of the leading points in our position, and attacking other points in a form in which we have never attempted to defend them, nor indeed ever put forward. By far the most remarkable features of Dr. McDougall's reply to my article are his omissions. He makes no reference whatever to many of my most fundamental arguments, which, therefore, I can only suppose he is prepared to admit. To avoid a similar accusation on the part of Dr. McDougall, I shall now endeavour to deal in order with every point raised in his article, and I shall afterwards enumerate those points in my previous article which Dr. McDougall has neglected to notice in his reply.

The first point is concerned with the nature of the evidence required to establish the existence of a vital force. I had pointed out the absence of any positive evidence, and had commented on the fact that all the so-called proofs of vitalism were merely cases in which mechanistic explanations were difficult to imagine : and I

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urged that this constituted no evidence in favour of vitalism. To this Dr. McDougall replies :—

“What would he have? Does he demand that the non-mechanical factor or factors—call it for convenience ‘vital force’—shall be exhibited in a bottle of spirit, or in a series of microscopic sections, or otherwise presented to his senses?”

Now this is scarcely fair: for I definitely stated in so many words what in my opinion *would* constitute a proof of vitalism.

“The vital force,” I said, “would be definitely established, by production of a case in which matter has been set in motion under conditions which absolutely exclude the possibility of a mechanical agency.”

Dr. McDougall’s wit about putting the vital force in a glass bottle is therefore somewhat misplaced: it is analogous to Dr. Johnson’s refutation of idealism by kicking a stone. But let me continue the citation from Dr. McDougall’s reply :—

“Does he not know that many—in strictness, all—the things and forces or energies by whose aid the physicist explains the flux of phenomena are imperceptible; that they are not perceived, but only conceived; that these conceptions have been achieved by an effort of the imagination; and that the only justification for any conception of this kind is that it fills a gap in the system of explanation better than any other conception?”

Yes, I am fully aware of the fact, and if I had said that force was a kind of matter, the criticism would have been relevant—but I did not. Surely Dr. McDougall perceives the profound difference existing between the mechanical forces of the physicist and the vital force of the metaphysician. The former is a conception about which specific scientific laws can be formulated. The sciences of statics and dynamics exist for the purpose of stating the resultant action of the forces which must necessarily be in operation in a given limited system of matter and motion. In short, physical forces obey fixed and unalterable laws: they are of universal application; and the motion resulting from them is calculable and certain, in all cases where the data are sufficiently simple to come within the range of mathematical analysis.

The vital force has none of these qualities. No laws can be attributed to it: so far from being controllable or of universal application, it is known to be almost universally *in-applicable*: it

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is an *ad hoc* hypothesis invoked to "fill a gap": and it does not even fill the gap for which it was invented. As I previously insisted, the entire progress of natural knowledge has been correlated with a displacement of spiritual forces by physical forces. In primitive times, unexplained phenomena were attributed to spiritual forces of arbitrary, fickle and unprophesiable character. With the progress of science they were replaced by physical forces of definite, permanent, universal and prophesiable character. The displacement of spiritualistic forces has proceeded to such an extent that no one now desires to invoke them, save in the most complex group of animal reactions: the only class of phenomena still remaining where the mechanical nature of the forces at work are not completely manifest. It is but recently that spiritual or vital forces were dispelled from our explanations of the simpler animal reactions, such as reflex action. And because a minority of physiologists are not yet clear as to how mechanical forces can be operative in the more complex reactions, therefore we are asked to assume gratuitously that there is a gap to be filled, and the vital force is fetched out from nowhere and alleged to fill it: a force which explains nothing, which is discredited by its ancestry, which never yet has had a victory in conflict with a scientific law, whose history is one of unbroken defeats, and of expulsion by degrees from the office of explaining every phenomenon of the universe to the office of ministering to the vulgar superstitions which still linger among the ignorant. Such was my argument: and in reply Dr. McDougall inquires whether I expect the vital force to be exhibited in a bottle of spirit. No! Not unless the bottle is deposited in a museum of curious primeval beliefs.

To meet my objection to any proof of vitalism founded on some set of facts which have not yet been explained in mechanical terms, Dr. McDougall presents a case in which the facts not merely have not, but (according to him) *cannot* conceivably be explained in mechanical terms. The case is borrowed from Hans Driesch, and Dr. McDougall quotes it as follows:—

"The mechanist regards the development of the egg to the form characteristic of the species as determined at every step by the physico-chemical structure of the egg; i.e., by the constitution or nature of the material particles which make up the substance of the egg, and by the spatial distribution and relations of these

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particles within the egg. Yet the spatial distribution of these constituent parts of the egg, or of the embryo at various stages of development (and therefore their reciprocal influences upon one another), may be (and in some experimental instances have been) profoundly altered, without preventing the development of the egg to a complex multicellular organism having all the characteristics of the species."

I have, in compliance with Dr. McDougall's request, "pondered this argument" with all care: I have reconsidered my treatment of it in *Science Progress* of last January, where he says I have "failed to grasp the point of it": and as a result of these reflections, I am still completely baffled as to whereabouts the point may be: the argument still appears to me destitute of even the appearance of force. Driesch dealt in sea-urchins: he cut off a part of the embryo: yet the remainder developed into a pluteus, just as if no part had been cut off. And this is alleged to be an impossibility under merely mechanical laws. But I cannot conceive why! Take a crystal and mutilate it in various ways. It will develop again to an adult crystal "having all the characters of the species": if that can be done by physical forces, why not also in the case of the sea-urchin? Of the various existing materialist theories of organic development, I cannot think of one which would be in the least degree affected by Driesch's experiment. Take, for instance, Herbert Spencer's theory of constitutional units,* which has always seemed to me to be probably as near a guess at the truth as the present state of knowledge will permit. Under this theory the material particles of the egg or the embryo might theoretically be disturbed to almost any extent, and yet the remaining portions (if they survived) would develop to the normal adult form. So far from Driesch's interesting experiment throwing any obstacle in the way of a materialistic theory of development, all the previously-existing materialistic theories with which I am acquainted would remain untouched by far more fundamental mutilations even than those practised by the German embryologist. If Dr. McDougall asks me to name the actual physical and chemical forces operating in organic development, I cannot, for the causes of development are still obscure: but that the causes, when discovered, will be found to be physico-chemical

* The "physiological units" of the earlier editions of the *Principles of Biology*.

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in nature (like all other causes known to man) I cannot see the slightest reason to doubt.

In this paragraph, I take exception to one citation which Dr. McDougall makes from me. He says I am "an uncompromising materialist ; for him ' all matter is reducible to atoms, and all energy to matter in motion,' ' we may look upon an atom in motion as the unit of all physical phenomena.' " Now why does Dr. McDougall make two citations of what in my original was all one sentence, and why does he miss out the important word " if " with which I opened the sentence ? Anyone reading the citation would suppose that I was ignorant of modern theories of the constitution of matter, in which the atom is regarded as divisible into far smaller units : a theory which I received with enthusiasm from its first announcement. What I was speaking of was cerebral processes, "*if reduced to their last chemical analysis.*" And under this definitely-stated proviso, I venture to think that my statement about atoms and energy is not a controversial one. I was under the impression that I was stating an unquestionable truth ; equally agreeable to the materialist or the idealist. Although I have no particular objection to being called a materialist, I cannot see how I am entitled to that denomination by having happened incidentally to recite an elementary principle of physics.

We now come to my attack on vitalism on the ground of its infringement of the law of the conservation of energy. " Mr. Elliot," observes Dr. McDougall, " puts aside the opinion expressed by a number of eminent physicists to the effect that this law does not rule out mind or spirit from all participation in the course of events." Now, on these lines, I might if I chose retaliate by saying that Dr. McDougall puts aside the opinion expressed by a number of eminent physicists to the effect that this law *does* rule out mind or spirit from all participation in the course of events. But I prefer to take higher ground altogether : I venture to think that the matter is sufficiently simple for us to decide all by ourselves, without any assistance from eminent physicists.

Explanations of all inorganic events are based upon the assumption of matter being a fixed quantity, changeable only from one form into another, and of energy likewise being a fixed quantity, similarly changeable from one form to another. The various forms assumed

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by energy are the subject-matter of the different departments of physics, and the laws of its transformation from one form into another are thoroughly understood. Now it is a simple question : have physicists found that just as there is an energy of motion, an energy of heat, an energy of light, etc., so also there is an energy of life ? Have they found that just as a definite quantity of motion-energy can be changed into a definite quantity of heat, so also it can be changed into a definite quantity of life ? Have they found a mechanical equivalent of life, as they have of heat ? When some quantity of energy in their laboratories disappears, do they ever find that it has gone into life-energy, or do they find with invariable certainty that it has merely assumed some other form of physical energy ? The proposition is uncontested : energy never disappears : whenever it seems to disappear, its equivalent may always be found in some other non-spiritual form : nor does energy ever appear out of nothing. Now where is there any room for the vital energy which some eminent physicists are not prepared to rule out ? If they believe in it, let them find it, instead of talking about *à priori* possibilities. Let them show us some physical form of energy disappearing into an organic form without the production of any substitute. *Then* there would be a case for believing in a new form of energy, called vital. Or let them show physical energy evolving out of an organic form in quantities greater than had previously been put in. *Then* we should begin to believe in vital energy. But no such cases are produced. Although mankind handle daily inconceivable quantities of energy, although the lives of a large proportion of the population are passed in doing nothing else, yet no single case has ever been placed on record where energy has vanished unaccountably, such as would happen if it were to assume a "vital" or "spiritual" form. Nor has energy ever been known to appear out of anything but pre-existing energy : and we may be sure that if such a thing happened, it would attract the widest attention. For energy has a definite commercial value, and if anyone possessed the capacity to create "vital energy" out of nothing, he could turn his endowment to a most profitable issue.

Dr. McDougall then goes on to analyse my argument into the following form :—

"Every form of influence that operates in the world is properly

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called force. All force is the motion of particles of matter. Therefore the course of all events is wholly determined by the reciprocal influences of particles of matter, and there can be no other types of process, activity, or influence in the universe."

Now this is such an extraordinary travesty of my argument that I find it very difficult to understand how Dr. McDougall can possibly have read into my article such rubbish as he here puts to my credit. To represent me as having said that "all force is the motion of particles of matter" is the more inexcusable, in that I carefully defined what I meant by force, saying that it "is a name for the influence by which any portion of matter tends to alter the direction of motion of any other portion." But that force *is* motion is a proposition which, even as an Eton boy, I should have laughed at : and which I fancy no scientific materialist has ever suggested. One might as well say that the shepherd, who drives a flock of sheep to market, must himself be a sheep. It is a misfortune that none of the "eminent physicists" mentioned by Dr. McDougall, who deliver *ex cathedra* utterances in favour of spiritualistic physiology, can be induced to come and discuss the whole subject on their own ground, starting from the law of the conservation of energy and Newton's laws.

The other above-cited passages of Dr. McDougall I equally repudiate. "Every form of influence that operates in the world is properly called force." The vagueness of this is such, that I do not in the least know whether I should support it or not, if its significance was made definite. "There can be no other types of process, etc." I never took this *à priori* ground. I only said that there was no evidence of other types of process.

On the subject of teleology, Dr. McDougall again misrepresents me. For he attributes to me the assumption "that natural selection is all-sufficient." I neither said so, nor for some years past have I thought so. Of the various hypotheses for accounting for evolution, it is the only one which in my opinion is proved beyond doubt to be a *vera causa*. But that it accounts for everything appears to me less probable, though possible : and I await the discovery of other factors, of materialistic nature, which shall advance our knowledge of the causes of evolution.

I now come to Dr. McDougall's attempt to establish natural

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selection itself as being spiritual in character. In my former paper, I quoted two passages of this kind from Dr. McDougall, one that "natural selection presupposes the struggle for life among organisms," and the other that this struggle "is essentially a psychical struggle in that it presupposes the 'will to live.'" In reply, I cited the cases of leaf-insects and carrion flowers. The carrion flowers depend for their fertilisation on the visits of carrion-loving flies: the odour was developed by natural selection, and I asked Dr. McDougall wherein this struggle was psychical, and in what respect it presupposed a "will to live" on the part of the carrion plant: the materialistic explanation of course being that the more it smelt of carrion the more certain was it of attracting flies and being fertilised and carrying on the race: while those which smelt less would be propagated less and become a diminishing factor in subsequent generations. Dr. McDougall's reply to this question consists of two lines. "I reply that, looking like a dead leaf, or smelling like carrion, is not an exhaustive enumeration of the activities of the organisms in question." I am at a loss to understand how this reply is in any way relevant. Dr. McDougall affirms that natural selection is psychical and presupposes the will to live. I then adduce instances of natural selection in the concrete, and invite him to specify in them the psychical element and the manifestations of a will to live, which he affirms to be presupposed by natural selection. And he rejoins that the organisms I named have other activities besides those under discussion. If readers of BEDROCK grasp Dr. McDougall's point, they are more fortunate than I am. Until Dr. McDougall has pointed out to me in these instances of natural selection (accepted by him) where the psychical element and the will to live become apparent, or where indeed there is any room for their interpolation, I shall remain convinced that neither one nor the other of them has anything whatever to do with the matter.

In the following paragraph, Dr. McDougall comes to the question of machines. He has misunderstood the grounds for which I adduced instances of machines in inorganic nature, independently of human purpose. He imagines that I was attacking his "contention that to liken an organism to a machine does not deprive it of its purposive character": but I was not attacking that conten-

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tion : on the contrary, I consider that, from the teleological point of view, the two things are on much the same plane : and if you call the one purposive, you must also call the other purposive. As I observed in my previous paper, anything may appear purposive if you take the appropriate mental point of view. But the reason why I adduced instances of machines in inorganic nature was that Dr. McDougall affirmed that there were no such things. For some reason, he has still not withdrawn his expression of opinion. He describes my argument as " the merest quibbling " : but that does not help matters. I regard most of what Dr. McDougall writes as the merest quibbling, and if that sufficed as an answer to his book, I should have saved myself a lot of trouble. But unless some attempt is made to show how, where, or when I have quibbled, I shall continue to look upon my argument as unanswerable : and to make the only rejoinder possible on the same intellectual plane, namely that I have not quibbled, and that Dr. McDougall has.

I now reach the point where Dr. McDougall thinks I have quoted unfairly from his book, in an imaginary case of automatic writing. What he says is this :—

" I went out of the way to show that those are wrong who maintain that it is impossible to imagine events which would constitute convincing evidence that human personality may survive the death of the body. I did this by constructing an imaginary series of incidents which, if they were to occur, would, it seemed to me, constitute such evidence. Mr. Elliot cites for the amusement of his readers the most extravagant incidents of my imaginary case ' as an instance of the extremity to which vitalists are reduced in their attempt to find arguments against mechanism ' ; and he adds ' I am not going to comment on this passage. ' I would suggest that this is hardly ' cricket. ' It might, or might not, pass as fair in the law courts, " etc., etc.

I cannot admit the impeachment : I shall therefore reproduce my original criticism as before, and I shall differ only by commenting on it. I referred to Dr. McDougall's note on p. 348 of his book *Body and Mind*, which runs as follows :—

" After the death of an intimate friend you seal up a pencil and a writing-block in a glass vessel. Then, whenever mentally or verbally you address questions to your deceased friend as though he were beside you, the pencil stands up and writes upon the paper, giving intelligent replies to your questions."

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Unfortunately this has not actually been known to occur, but something just as good happens.

“Pencils do produce what seem to be messages written by deceased persons ; but in the observed cases the pencil is held and moved by the hand and arm of a living person, who, however, remains ignorant of its doings and of the thought expressed in the writing. This fact that the pencil is moved by the hand and arm of a living person, complicates immensely the task of evaluating the significance of the writing, but does not in principle affect the validity of the inference that may be drawn from it.”

Now here Dr. McDougall offers us firstly an imaginary case, secondly an “observed case,” and thirdly the assertion that the same inference may be drawn from either. I made no comment before, because I thought it perfectly obvious that the same inference could not be drawn from either, and I thought that 99 per cent. of readers would not require that to be pointed out to them. If a pencil, experimentally isolated, begins to write of its own accord, we are in the presence of so singular a circumstance that it would if established go far to justify the fancies of the psychical researchers. But that it should write when held by a human hand is a phenomenon of a totally different order, even though the writer should by chance be telling the truth when he says he is not conscious of what he is writing. The fingers do much of which the mind is at the time unconscious. Women knit, musicians play, while thinking or talking of other things. Expert typewriters actually prefer *not* to be fully conscious of the words they are typewriting with their fingers. Their fingers travel perhaps a dozen words behind their consciousness : that is to say their full attention is fixed upon the “copy” : they decipher it and make out its meaning about a dozen words ahead of what they are writing unconsciously with their fingers. That impression then sinks down, as it were, through the nervous system to the fingers, where it gives rise unconsciously to the correct letters. The same is the case in operating the telegraph or the heliograph, as I can testify from personal experience. In short, there is nothing in the least unusual in the fingers carrying out intelligent and highly-trained movements in a perfectly automatic manner. But surely it is a far cry from such automatic writing to pencils getting up and writing of their own accord. The typist may not be aware at the moment what she is writing ; but

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that is a very different thing from the machine beginning to write when there is no typist about. I have often wondered why certain psychologists should be so surprised at automatic writing. I suppose that its explanation is attended with difficulties to a vitalistic psychology : to a mechanistic psychology it is scarcely more remarkable than ordinary writing. Dr. McDougall thinks that it is not "cricket" to make these citations from him. I hope he will now perceive what I was driving at : for my own part I confess that when I am dealing with such strange and unfounded opinions, my sensations resemble less those of a cricketer than those of a footballer.

Finally Dr. McDougall accuses me of having abandoned the Epiphenomenalism upheld in my *Modern Science and the Illusions of Professor Bergson* in favour of Psychological Monism : and his "accusation" is just : but I venture to think it is altogether academic and irrelevant with regard to the question in hand. When I wrote the book, and when I subsequently wrote the article, I had in mind almost exclusively the defence of physiological mechanism, by which all activities are explained without reference to mental or spiritual processes. The question as to where you are going to stow the "mind" in your explanatory system is altogether subordinate : the main fact for which I was contending was simply that it was inoperative in organic events. In writing my book, therefore, I gave no individual consideration to the subject. I adopted Huxley's theory that the mind accompanies the cerebral processes as a shadow : and I adopted it partly because it is the simplest of all parallelistic theories and the most easily accepted by the untrained public, partly because it was the only one with which I was acquainted at the time. It is true that I now prefer a slightly different form of parallelism or rather of monism. I regard Epiphenomenalism as a first approximation to the truth ; and that I believe is all that Huxley ever claimed for it. I still prefer Epiphenomenalism to any other theory which I have seen published ; and I much prefer it to Psychological Monism, if by that is meant the theories of W. K. Clifford or the doctrine of Paulsen's *Einleitung*.* As I hope to publish within

* I wish to make an exception in favour of William James's doctrine of Radical Empiricism. Just as materialism in history has always been associated with

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the next twelve months an elaborate discussion of the whole problem, I need say no more now except that Epiphenomenalism, though not literally tenable, still seems to occupy the field as the least untenable of existing theories, and is believed by most physiologists in that sort of tentative way. I hold that it is at present the most convenient hypothesis for representing the various parallelistic theories.* That Dr. McDougall is of a similar opinion, I gather from the footnote on p. 330 of *Body and Mind*. For in that chapter he discusses at length the problem of memory in special relation to Epiphenomenalism, as representative of all forms of Parallelism : and he does this although admitting, or rather affirming, that it is not the most tenable form.

Finally Dr. McDougall complains that I have ignored many parts of his argument against Psychological Monism, and that by harping on the wearisome old refrain "vital force explains nothing," I have seriously misrepresented his work. I am glad that vitalists are beginning to find this refrain wearisome, and expect they may find it more wearisome still as time goes on : for mechanists are likely to continue to harp upon it, until vitalists have recognised that it is true. As for Dr. McDougall's attack on Psychological Monism, I fail to see why I was called upon to answer it. We are discussing at present simply the question whether vital processes are or are not completely accountable in physico-chemical terms : this is a purely scientific question : Psychological Monism on the other hand is a hypothesis as to the relation of mind and body : and it *assumes* the truth of physiological mechanism as a basis. When mechanism is accepted, the further question then arises as to what relation mind bears to the material system of the universe. But it is impossible in an article to deal with both questions. Dr. McDougall will recollect that his book consists of 379 pages, and my article of twenty pages, of which only twelve were mainly devoted to Dr. McDougall, the remaining eight being devoted to other writers. Although,

empiricism, so I have no doubt that the modern scientific materialism will come to be associated with radical empiricism.

* Strictly speaking, it is a contradiction in terms to call psychological monism or any other identity-hypothesis a theory of parallelism. But it is so convenient to have a generic name for all these theories deduced from physiological mechanism, that I have followed Dr. McDougall's precedent in so using it.

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therefore, I did indicate that in my opinion, mechanism led on to a monistic theory, I did no more than sketch such a theory as it presented itself to me, without attempting either to give arguments for it or to meet arguments against it.

I here conclude my reply to Dr. McDougall's criticisms. I believe I have conscientiously touched upon every point raised by him. It now only remains for me to recite the points of my former paper, and to note those to which Dr. McDougall has not been able to make any reply.

(1) In his book Dr. McDougall endeavoured to show that vitalism was not incompatible with the law of the Conservation of Energy. I replied with an argument lasting over several pages, summing up my argument as follows :—

“ If cerebral processes involve something more than mere mechanical interplay, then you must assume one of two things : either that atoms alter their *velocity* of motion, without external cause, in which case you traverse the law of conservation of energy ; or that they alter their *direction* of motion, in which case you traverse Newton's laws, and the law of conservation of matter.”

No reply.

(2) I attempted to show that “ all ideas of spirit, ghost, soul, etc., are in reality materialistic ” in the old crude form.

Reply : “ A matter for the editors.”

(3) I pointed out that no positive evidence of any kind had ever been offered in favour of the existence of the vital force.

Reply : “ Do I wish it exhibited in a glass bottle ? ” My rejoinder : “ Yes, in a museum of curious primeval beliefs.”

(4) I urged that the vital force explains nothing.

Reply : “ Wearisome old refrain.”

(5) Dr. McDougall had affirmed that Natural Selection is itself psychical and spiritualistic in essence. I thereupon brought forward the first undoubted instances of Natural Selection that occurred to me, and invited Dr. McDougall to indicate where their spiritualistic factor lay.

No reply.

(6) Purpose is subjective, arising from our way of looking at things ; not objective and existing in the things themselves.

No reply.

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(7) Dr. McDougall having alleged that there are no such things as machines in inorganic nature, I proceeded to adduce several instances of such machines.

No withdrawal.

(8) Dr. McDougall having cited two cases of pencils writing automatically : the first being an imaginary case in which the pencil was not held by a hand, the second an actual case in which it was held by a hand : and Dr. McDougall having affirmed that the fact of the pencil being held " does not in principle affect the validity of the inference that may be drawn," I reproduced the passage without comment.

Reply : " Hardly cricket." My rejoinder : " Football."

Finally, I summarised the arguments against vitalism in a series of twelve numbered propositions. To my great astonishment Dr. McDougall has not offered any reply to a single one of them. We are witnesses then of the spectacle of one of the most competent living defenders of vitalism, in an article expressly directed against a mechanistic attack, unable to face more than a minute proportion of the facts brought forward in that attack, and allowing the bulk of the arguments to go by unquestioned. Surely we are justified in inferring that vitalism is no longer a living issue : that it cannot come out into the open : that it will not meet the crushing arguments opposed to it, *because it cannot meet them*. It has broken down on all sides where attacks have been levelled : from the side of physics, from the side of biology, and from the side of physiology. Already its adherents are few among trained scientific observers. The twentieth century will not have advanced much further before it begins to lose its hold on mob-popularity ; for even those melodramatic and thaumaturgical instincts, which pullulate in every public assembly and in every newspaper, will be inadequate to sustain a hypothesis so hopelessly discredited by scientific analysis.

Dr. McDougall, in his article in reply to mine, makes an attack on Sir Edward Schäfer and Professor Loeb : and I have no desire to trespass on the privilege of these gentlemen to reply, should they wish to do so. I note however, that Dr. McDougall, with

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reference to a remark of Sir Edward Schäfer, recommends the biologist occasionally

“to make an excursion to the nursery, where he may hear pronounced by the fresh voice of childhood some of the old riddles which the mechanistic scheme leaves as insoluble as ever. ‘Where does space come to an end?’ ‘When did time begin?’ ‘What was there before the world began?’ ‘Why can’t I stop thinking?’”

I wish to ask when and where the “mechanistic scheme” has undertaken a solution to such questions as these. And if the “mechanistic scheme” leaves them insoluble, I wish to ask where I may find the solution proposed by the “vitalistic scheme.” I had previously looked upon mechanism and vitalism as rival theories of neural physics: and therefore in no way connected with impossible questions such as the beginning of time. And then what about the “fresh voice of childhood,” with its implied contrast to the stale croaking of the materialist? Has Dr. McDougall to go into the nursery to be inspired with such questionings as these? Surely the adult philosopher is far more overwhelmed at the thought of these ultimate mysteries, than any child can ever be. For he knows, as the child does not know, that they are for ever insoluble and beyond the range of the human mind: he suspects that, not only is there no possibility of comprehension of them by the human mind, but that there is no comprehension of them anywhere. Truly the marvellings of the child’s nursery are bathos after the philosopher’s study.

And yet this remark of Dr. McDougall has filled me with depression. If a learned man of scientific authority and trained judgment, will nevertheless attack Sir Edward Schäfer by depositing on his trail a red herring of so stinking and palpable a character as this, what hope is there for the general public? The solution of the vitalist problem is of vast importance for humanity. On it rests not only a true philosophy, but a true sociology, a true educational system and (as Loeb has said) a true system of ethics. Everything connected with human conduct or activity rests upon it. And if a man of Dr. McDougall’s attainments can bring himself so to misconstrue the facts, what hope can there be of effecting improvements in the outlook of the general public?

THE TRUTH ABOUT TELEPATHY

By a Business Man

It is sad to see the Principal of a University in a large commercial City stooping to abuse, simply because an honest and fairly successful attempt has been made to undeceive large numbers of our fellow creatures who, being neither scientific nor business-like, have hitherto accepted as Gospel that which is, after all, merely the expression of an opinion ; and the moment seems opportune for putting on record a few facts which may not be out of place in a " Review of Scientific Thought."

Near the foot of p. 60, in the April number of BEDROCK, Vol. II., Sir Oliver Lodge wrote :—

"The business man takes another line and offers a thousand pounds for proofs which will convince him. *He has, of course, no intention of parting with the money, and is quite satisfied that he can resist any temptation to be convinced.* . . . A thousand-pound note is a weird argument in Science. . . . To all wagers of this kind I trust that those connected with the S.P.R. will always turn a deaf and contemptuous ear."

I do not know what evidence there may be of " wagers " ; but let me ask, in all seriousness, what are the reasons for making the defamatory statements which I have put into italics ? They certainly are not true, if intended to refer to a case I have in mind ; and they are unworthy of the Man of Science to whom, in all good faith, liberal payment was offered for particulars supposed to be available but which he failed to supply. He had often declared that, to him, Telepathy is " perfectly clear and certain " ; and nobody ought to doubt the sincerity of Sir Oliver Lodge's belief ; but, requiring facts and not beliefs, I waded through volumes of " records " to which he had referred me, and worked back to his

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first experiment with a Square and a Cross, as described by him in a letter to the Editor of *Nature*, dated June 12th, 1884. Being unable to find anything but statements that would not bear to be looked into carefully, or accounts of phenomena that had occurred under conditions in which trickery was always possible, I pressed Sir Oliver Lodge for definite information, and finally got from him this gem :—

“I am surprised that you imagine that incontrovertible evidence can be obtained at all in an inductive problem.”

After writing to many others, I caused the following advertisement to be inserted in *The Times* for several days in August, 1911 :—

“ TELEPATHY.

“The sum of £1,000 has, during the past six months, been offered privately to the leading authorities and writers of repute on this subject for satisfactory proofs of so-called Thought-transference, but not one single case could be found ; and it has now been decided to advertise publicly for the particulars required. Persons applying to the undersigned are requested to name their own terms for evidence that will stand cross-examination, and to state whether or not their communications are to be treated as confidential. MATTHEW JARVIS, Solicitor, 4, Finsbury Square, London, E.C.”

There is no suggestion of “wagers” in the above,—only a plain statement of fact and a request that persons shall name their own terms ; but, though the replies were too numerous to acknowledge the receipt of separately, no *evidence* could be obtained. The advertisement was copied into many Foreign and Colonial Newspapers, so that dupes all over the World were put on their guard against believing something that might not be true.

Remembering that Sir Oliver Lodge had specially vouched for the movements of a chair in the moonlight, on one occasion when he had tried to control a female medium for hours at a stretch, I next proceeded to arrange for the payment of £5,000 to anyone who could perform or prove a case of Levitation ; and, on my informing Sir Oliver Lodge of this, he wrote :—“To me these offers of money seem quite preposterous and never likely to obtain anything at all.” So I took this as an indication that he was again unable to produce any proof. But, supposing I offered him a

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large fee for writing a book on some mental incursion into the realms of the unverifiable, would there be, in such an offer, anything objectionable to which a deaf and contemptuous ear should be ever turned ; or would it be readily accepted by the Man of Science acting for the time being as a business man ?

Others, who are better qualified than I am, have dealt with the historical part of the subject, but it so happens that I had experience in the early seventies of last Century, in Germany, of a game called "Willing," which was imported by Schoolgirls into England, where it became fashionable even among Clergymen,—the daughters of one of whom living in Derbyshire completely took in Professor (now Sir W. F.) Barrett who "discovered" it some years before the S.P.R. was founded "with the establishment of Telepathy as its primary aim." Having known the Varleys and several of the original members, I can testify to their ready acceptance of a theory that helped them out of a difficulty about Ghost-clothes ; for, the sex of Ghosts being determined by their dress, when awkward questions were asked bearing on the resurrection of wearing-apparel, we were told that the Power who produced the Spirits might be trusted to see to their being decently clad. Telepathy made things easy, for, by it, a Ghost was no Ghost,—only the visualisation of an idea impressed upon the mind of the observer by another mind ; and, even as a business man, I do not mind admitting that, over thirty-five years ago, this style of reasoning appealed to many ; but, having grown older and less credulous, it now appears strange to me that present-day members of the S.P.R. should attach any importance whatever to opinions and expressions of belief, instead of trying to prove facts. Perhaps they do not consider that there is any credit in believing what is merely true,—because any fool can do that,—and they find comfort in the Bergson-Lodge philosophy which has become fashionable because it is so "scientific" ! You start by accepting something which may or may not be true, but which is highly improbable and has not been proved ; and, on such a want of basis, you build up your system,—making everything as obscure as possible in order that it may seem profound. As an example of what a practical politician can do when he gives rein to his imagination, let me cite the Rt. Hon. G. W. Balfour, V.-P. S.P.R., who has an extraordinary article in the *Hibbert Journal*

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for April on "Telepathy and Metaphysics," from which I make a few short extracts :—

" . . . for the purpose of the present paper I must be content to assume that Telepathy . . . is an established fact ; . . . if the transmission of an idea from one mind to another is to be ascribed to Ether waves, as suggested by Sir William Crookes ; . . . once admit that Telepathy . . . actually takes place between one individual and another, . . . If Telepathy is the fundamental law of conscious life ; . . . if telepathic interaction is universal, as we have supposed, . . . Telepathy, the reader will have noticed, has grown and grown until, under the name of 'awareness of other' it has threatened to extend to the entire field of Being."

After so much assumption, suggestion, assertion, and relying on opinion, no business man need be surprised to find that, towards the end of the article, Mr. Balfour confesses boldly : " For myself, I am a believer in the Soul theory,"—which expression of belief will be mistaken by those whom it is intended to influence as proof positive of something they wish to believe. But surely it is not scientific, for a Man of Science, to talk of Thought-transference without first showing reasonable probability of thought being something that can be transferred, or giving some reason for supposing that, like Light, Thought is somehow due to Ether waves ? Yet Sir Oliver Lodge himself has gone further than this in stating, in one of his *Hibbert Journal* Essays on the " Immortality of the Soul," that " motion is *transferred* from one body to another," which is actually *untrue*, because Motion is not a thing, nor an entity ; so it cannot be transferred any more than can Life, Mind or Consciousness,—which are states or conditions ! If our S.P.R. friends could only realise this plain truth, there would be fewer of those so-called arguments which, being neither sound enough to be called logical, nor weak enough to be quite illogical, are merely what I ventured to describe some years ago as " olodgical " ; and, having thus immortalised a Man of Science, the humble business man is vilified in such unworthy insinuations that I do not see how, to use Sir O. Lodge's words, " the issue can be brought into a Law-Court and decided." If he had taken my advice in 1894, Eusapia Paladino could never have imposed upon him as she did ; but he went out to Hyères feeling confident that there was something " psychic " in her Tomfoolery ; and, even after she had

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been detected in England, by others, in her fraudulent tricks, Sir Oliver Lodge still declared his belief that some of the phenomena were genuine,—which satisfied those who prefer pontifical utterances to evidence.

I can give an amusing instance of how some Members of the S.P.R. can be brought “to heel,” for when I approached Sir W. F. Barrett (whom I take this opportunity of again thanking for his kind promise of help) he was very keen to assist me in finding a case of Telepathy ; and he was good enough to offer to call upon me the next time he came over to England from Dublin. He made no objection to being paid £1,000 for proofs, and his letters show that he was genuinely anxious to be of use in the matter ; but, after seeing Sir Oliver Lodge in London, Sir W. F. Barrett wrote to me :—

“I could not undertake to prove the results of a long and difficult investigation to order or for a pecuniary offer.”

My reply was to point out that I had not asked for any long investigation, but that I merely wanted one single case of Telepathy, which he, as its Father or Discoverer thirty or more years ago, ought to have no difficulty in finding for me. I never got one, however, nor did I have any better luck with Sir William Crookes, who was too busy with other scientific work to help me.

I cannot but deem it unfortunate that the S.P.R. have not accepted Dr. Tuckett's offer of £1,000 * for a repetition of the Crookes-Fay “psychic” experiment, because their inaction may easily be misunderstood as implying their belief that this “experiment” established a “fact.”

I have no desire to advertise myself ; and my name need not appear, though it is known to Sir William Crookes, Sir Oliver Lodge, and many others who are interested in this matter ; but I shall be happy to place at their disposal all the information I have collected and to assist, as a business man, in getting at the facts. These, so far as they go, certainly confirm the statement of Sir Ray Lankester, on p. 66 of the April number of *BEDROCK* :—“I say that Sir Oliver Lodge and his associates have not (in answer to the question ‘Does telepathy exist ?’) given any demonstration of its existence nor even any evidence which makes its existence probable.”

* See *Westminster Gazette*, February 8th, 1913.

THE "MENTAL DEFICIENCY" BILL AND ITS CRITICS

By Sir Bryan Donkin, M.D., F.R.C.P.

IN a short article on "Legislation for the Control of the Feeble-minded," published in the July number of this Review in 1912, I called attention to certain misconceptions and misrepresentations underlying much of the hostility to the Mental Deficiency Bill which marked the Parliamentary discussions of last year. The Bill has been reintroduced this year, in a revised form with some rearrangement and certain amendments and omissions.

The chief object of my previous article was to emphasise the fact that the Mental Deficiency Bill, as well as the Report of the Royal Commission of 1908, on which the Bill was mainly based, was primarily intended to secure proper care and control of such individual "defective" persons who were neglected and uncontrolled, and thus both suffered themselves and were free to inflict multiform harm on others. Reference to that article will, I think, satisfy the reader that it is wholly erroneous to represent this measure as essentially "eugenic" in aim, or as directed primarily to the prevention of procreation by feeble-minded persons on the ground of the transmissibility of mental defect.

In repeating this contention here, and especially in view of a somewhat important amendment in the new Bill, presently to be noticed, it is necessary to remind the reader that although, in the body of their Report, the Royal Commissioners expressed the opinion that, owing to the high probability of the frequent and direct transmission of mental defect from parent to offspring, the segregation of certain feeble-minded persons would be likely, by incidentally preventing procreation, to have useful effects, yet no formal Recommendation was made by them for legislation with this

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specific object. And the reason for this express omission of such a recommendation was that the actually available knowledge on the matter of "transmission," in spite of its being massive and possessing a very high degree of probability, was not sufficiently conclusive to constitute, *per se*, a basis for legislation. In last year's Bill, however, a paragraph was introduced including (among certain defined classes of persons who might be dealt with) those "in whose case it is desirable, in the interests of the community, that they should be deprived of the opportunity of procreating children." The insertion of this paragraph, though doubtless gratifying to a certain section of the supporters of the Bill, was, probably, the origin of the false charges of opponents that the whole measure was "eugenically" inspired, and was mainly directed to prevent the procreation of the feeble-minded; and, further, it was likely to elicit the more appropriate criticism that the scientific basis for such an enactment was not sufficiently established.

From the Bill now before Parliament this paragraph has been omitted, to the satisfaction of a large number of the Bill's supporters. It is recognised that some enthusiastic Eugenists have confidently announced the complete abolition of the future supply of "defectives" as the result of any measure precluding procreation by such persons at present existing; while it is well known to all who have carefully studied this question in all its aspects that no *practicable* measure could possibly secure this object, however much it might limit this supply. For it is certain that all grades of marked mental defect frequently appear, developed suddenly, and, as it were, *de novo*, in families where it is impossible to trace any near ancestor or other relative who is, or was, demonstrably affected in a similar manner. Doubtless it is the case that such "defectives," once they have appeared, tend, like other persons, to propagate their like; but, in view of the fact that there has been so much exaggeration in certain quarters regarding both the *effects* and the *practicability* of wide-reaching "eugenic" measures in this direction, it is felt by many supporters of the Bill that the omission of the debatable paragraph in question serves to remove all grounds for rational opposition.

No such opposition, surely, can be offered on the ground that the enormous mass of evidence collected by the late Royal Commission,

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both from oral evidence and from their own specially appointed investigators, failed to justify the conclusion on which their Report and the present Bill are based. This conclusion was that there are “*numbers of mentally defective persons over whom no special control is exercised, and whose wayward and irresponsible lives are productive of crime and misery, of much injury and mischief to themselves and others, and of much continuous expenditure, wasteful to the community and to individual families.*”

Opposition, however, of other kinds will, doubtless, persist or arise. The perverse iteration of the never-defined phrase—The Liberty of the Subject—will still be the text for preaching that nothing but actual crime already committed by a person not certified under the existing lunacy laws can justify the segregation of anyone, whether actually neglected or not, however strong the probability may be, from the previous history of any individual case, that he will continue to repeat such crimes or such mischief as he has already perpetrated, or that he will remain in demonstrable conditions of misery, neglect or ill-treatment.

Recently another kind of objection to this Bill, of a less purely sentimental character than the foregoing, has been published in an article, written by Dr. Bernard Hollander, of which a copy, taken from the *British Review*, was lately sent to me. The author, among other misconceptions, makes the demonstrably erroneous statement that the main object of this present Bill is “to carry eugenics into practice.” Agreeing, nevertheless, with the “intentions” of its “promoters,” and granting his “favour” to the “principles underlying” it, he objects to the Bill as “premature,” though he hopes that “further-reaching measures will be proposed later on !” And the only tangible argument he adduces to substantiate this objection is that the Government has no right to segregate mental defectives unless there are physical signs of brain-disease discoverable in the size and shape of the sufferers’ skulls ! Space does not allow, nor does necessity demand, any serious discussion of this *dictum*, which is introduced by a paragraph inferring ignorance on the part of the Royal Commission that the brain is the organ of mind, from the Commissioners’ neglect of any mention of the skull in their definitions of mental defect. The acceptance of this new revelation, which virtually announces the portentous dogma that no observa-

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tion of conduct, or of any mental expressions of defective brain-function, can serve as tests for certification of the mentally afflicted, apart from demonstration of abnormalities in their skulls, must entail at once a vast "asylum-delivery" of the insane, a season of national humiliation for the wrongs done to them in the past, and an immediate revision of the lunacy laws in accordance with the doctrine of that esoteric super-phrenology which seemingly inspires Dr. Hollander's confident assertions.

I submit, in conclusion, that, although the Mental Deficiency Bill is limited in scope, and will probably require future expansion in some directions, it gives power to deal efficiently with many of the most pressing and salient evils that have been amply proved to exist; and I repeat that its main objects are the proper care of individual mental defectives, and the protection of the community from the numerous misdeeds and harmful actions committed by many of them.

THE NEBULAR HYPOTHESIS AND ITS DEVELOPMENTS

II. Miss Blagg's Substitute for Bode's Law

By Professor H. H. Turner, F.R.S.

THE suggestion of Miss M. A. Blagg (mentioned at the close of the first article as still unpublished) was put before the Royal Astronomical Society on April 11th, and is printed in the April number of the *Monthly Notices*. The following account is given in rather more general terms, and we will begin with an illustration from the commercial world which may provide rather more familiar mental images.

Suppose a sum of money were put out at compound interest and the rate of interest remained the same throughout. It is a familiar fact that the original sum will double itself after a term of years; double itself again after another equal term; and so on. The term of years required depends on the rate of interest, the following being the terms required for different rates :—

Interest per cent =	3	3½	4	4½	5	5½
Term of years =	23½	20	18	16	14	13

For simplicity let us start with £100 and assume the rate of interest to be 5 per cent. Then after fourteen years (actually fourteen years and 2½ months: but we will neglect the odd months) the sum will have grown to £200; after another fourteen years to £400; after another fourteen years to £800, and so on. This is on the supposition that the rate of interest is steadily 5 per cent.

Suppose now that we were uncertain about the rate of interest, but that on enquiry at the bank every fourteen years we learnt that the original £100 had successively doubled itself and become

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£200, £400, £800, etc., as above. We could infer with reasonable probability that the rate of interest had remained constantly 5 per cent. [There are, however, possible alternatives: the rate might have varied, being sometimes greater than 5 and sometimes less; but if so it must have varied in such a way that the excesses exactly balanced the defects every fourteen years; and though we might regard this as unlikely, perhaps in the highest degree unlikely, we must retain it as a possibility unless we can rule it out by independent evidence.]

Suppose, however, that on enquiry at the bank every fourteen years we find that the increase is *not* regular; that on the first occasion we find £208 instead of £200; on the next occasion, £432; on the next, £816; on the next, £1,536; on the next, £2,944, and so on. Then we can infer with certainty that the rate has varied; we see that at first it was more than 5 per cent. since the sum is more than doubled each time; and later it is less than 5 per cent. since doubling is not reached. We could compare our actual results with those of a steady 5 per cent. as follows:—

Steady	.	.	£200	400	800	1,600	3,200
Actual	.	.	£208	432	816	1,536	2,944
Difference	.	.	+ 8	+ 32	+ 16	— 64	— 256

But this is scarcely the best method of comparison, for the final deficit of £256, large as it is compared with the first excess of £8, is a deficit on a much larger total amount. It will give clearer information about the rate of interest and its fluctuations if we express the differences as *percentages* of the top line; thus:—

Difference per cent.	+ 4	+ 8	+ 2	— 4	— 8
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It will be seen, for one thing, that the second plan of comparison is much more comforting to the investor. The first plan suggests that he is rapidly running down hill, for though he was at one time ahead it was only by £32 at most; and he is left £256 behind. But on the second plan (of percentages) he sees that the 8 per cent. deficit is really no further from the steady line than the 8 per cent. surplus; and since the former corrected itself, there is reasonable hope that the latter may straighten out also.

We will make one further supposition and then we shall have made sufficient use of this mundane example. So far it has suggested to us that while the general rate of interest remains near

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5 per cent., there are fluctuations which leave us sometimes ahead of expectation and sometimes behind it ; with nothing particular to guide us to one alternative or the other. But suppose now that in another country and in a different century some one else invested another £100 at compound interest, and enquired at his bank every fourteen years and found that it had become successively :—

£204	416	808	1,568	3,072
------	-----	-----	-------	-------

so that on comparing as before with the regular series

£200	400	800	1,600	3,200
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we get the differences

+ 4	+ 16	+ 8	— 32	— 128
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which when expressed as percentages become

+ 2	+ 4	+ 1	— 2	— 4
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Now each member of this series is exactly half our former series :—

+ 4	+ 8	+ 2	— 4	— 8
-----	-----	-----	-----	-----

and we should be encouraged to make the following inferences from the pair of incidents.

Firstly, that the general rate of interest was in both cases 5 per cent., varied by fluctuations.

Secondly, that the fluctuations in the two cases were due to causes of the same *character*, differing only in *amount*.

The amounts differ in such a way that if we halve every item of the first set (or if we double every item of the second ; or if we draw diagrams of the two, making the scale of the diagram in the second case twice that of the first) we shall get identical results.

We have now illustrated the main points of Miss Blagg's suggestion with regard to planetary distances. The chief value of the illustration is to introduce the facts bound up with the ideas of percentage in a domain where they are tolerably familiar. The mathematical equivalent for these ideas is the use of logarithms, which might not be welcomed by those unused to them. There is a tradition of an old lady who once incurred the displeasure of a mathematician, so that he wished in a vague way to annoy her ; and the expedient which occurred to him in the excitement of the moment was to call her a logarithm, which is said to have produced an effect on her even beyond his hopes. In historic times, moreover (that is to say, within the last twenty years), it became necessary to report of another mathematician that some calculations about a comet,

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which had foreshadowed a result of great interest, had been afterwards found erroneous ; and the reporter added, by way of explanation, that the unfortunate calculator had “ detected an error in his logarithm.” Such incidents suggest that one should still be cautious in the introduction of the term in everyday life. And yet the invention of logarithms is almost as old as that of the telescope, and some would say that it has been of even greater value to the human race. The tercentenary of the telescope ought to have been celebrated (but was not) in 1909 at Middelburg, in Holland ; that of logarithms is to be duly celebrated next year at Edinburgh. In 1614 the first book of logarithms was published by Napier (or Neper) of Merchistoun, representing a truly wonderful effort of sagacity. An extract from a letter from Dr. J. W. L. Glaisher to the Tercentenary Committee is well worth quoting :—

“ More than forty years ago when I wrote my British Association Report on Mathematical Tables I was *intensely* impressed by the splendid effort of mind by which logarithms were invented, and the intervening years have only heightened this feeling. I have always felt that next to Newton’s proof of universal gravitation (and his showing that planets move under it in conic sections), Napier’s invention is the greatest contribution to science which has ever been made in these isles (of course I omit recent scientific discoveries about which it is too early to judge), and I always regard Napier as second only to Newton, and a close second.”

The primary use of logarithms is to convert multiplication into addition, and division into subtraction ; and this in itself is an immense gain. Take the problem we started with, to find in how many years a sum of money will double itself at 5 per cent. compound interest. We have to deal with a number of successive multiplications, since every year multiplies the sum by 105 hundredths, or 1·05 as we write it in decimals. Before Napier’s invention the only way of solving the problem was to keep multiplying by 1·05 time after time until the result came to twice the original, and (as we now know very simply) we should have to multiply fourteen times. But Napier substituted the following problem : how many times must we *add* a certain quantity to get such and such a result ? For simplicity, let us say how many times must we add 20 to get 300 more in all ? The answer is prompt—fifteen times ; and, though the details are just a little more complex, the general process is quite

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as simple for the actual problem before us, which is (in terms of modern tables), how many times must we add 21 to get 301 more in all? the answer being fourteen and something over. That is all that is necessary to find how many years are required for a sum of money to double itself at 5 per cent. compound interest when you have a set of logarithm tables to hand. And yet there are many country houses where even the library does not possess such a work! It ought to be on the hall table with *Bradshaw* and *Who's Who*.

Take another venerable problem: that of the nails in the horse's shoes. The buyer was to pay a farthing for the first, a halfpenny for the second, a penny for the third, and so on until he had bought thirty-two nails, when the horse was to be thrown in as a present. It sounded a cheap bargain until he began to work it out, and the working alarmed him long before he got to the end. It is, perhaps, too much to expect logarithm tables to be provided at a horse fair; but the logarithm of 2 is easily remembered (if we do not strain the memory with too many figures), being the above figures 301 with a decimal point in front. It does not stop automatically at the third figure; indeed, it never stops; we can calculate it to as many figures as we like, writing, for instance—

$$\log. 2 = \cdot 30102999566398,$$

and the row of figures on the right would still be incomplete; but practically the incompleteness makes very little difference. We can stop after seven figures, or five, or four, or even less, knowing that the result will still be approximately correct. In the present case we will stop at the very first figure, and take

$$\log. 2 = \cdot 3,$$

which is a fact easily remembered and often of great usefulness. To find now the cost of the thirty-second nail in the horse's shoes we have in the ordinary way to *multiply* by 2 thirty-two times; using logarithms we *add* $\log. 2$ the same number of times, making altogether $32 \times \cdot 3 = 9\cdot6$; and then at sight we learn—

(1) From the first figure 9 that there will be just ten figures (one more than 9) in the result.

(2) From the second figure $\cdot 6$ that the result will begin with a 4, since $\cdot 6$ is the sum of $\log. 2 + \log. 2$, and corresponds, therefore, to $2 \times 2 = 4$.

Hence the price of the thirty-second nail will be about 4,000,000,000

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farthings, or, say, £4,000,000. It takes, perhaps, a little time to explain this process, starting from the beginning as we have done ; it also takes a little time to ride a bicycle with facility, if we have never done so before ; but if we are content to spend a little time in learning either the bicycle or logarithms, they will enable us to reach results with delightful speed. It is curious how many of the great inventions have been devoted to the reduction of distances : the telescope, and the steam-engine, and the telegraph, and the bicycle. Perhaps it may seem strange to place logarithms in this list ; but if the telescope has shortened for us the distances of the heavenly bodies, the invention of logarithms has shortened quite as much the labour of learning about those distances.

And so we come back, after a rather long digression, to Miss Blagg's suggestion about those distances, which we will compare with our commercial illustration. The root-idea of the commercial illustration is successive doubling, and this was also the root-idea of Titius and Bode with respect to the planetary distances. But it was obvious from the first that some further modification was needed, since the actual observed distances of the planets (corresponding to the sums found in the bank-book every fourteen years), were not each twice the one before. Titius and Bode suggested, in explanation of the discrepancies, that there were really two sums of money at the bank : one on deposit at compound interest as already assumed ; the other lying idle and earning no interest at all. The addition of this idle sum (current account, we may perhaps call it) certainly explained anomalous features at first, while the sum at compound interest was still small ; but when this sum became large (i.e., for the more distant planets), the effect of the current account, as a percentage of the whole, became less and less ; and, when the outermost planet of all, Neptune, showed a serious departure from the law of doubling, the poor little current account was quite unable to meet the situation.

Miss Blagg therefore suggests, in the first place, that this notion of the current account, drawing no interest, has failed to meet requirements and must be abandoned. Let us return to the notion of a single sum at compound interest, and let us attribute the anomalies to variations in the rate of interest. We shall be dealing with percentages, therefore, or with logarithms which are their

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equivalent. Instead of dealing with the distances directly, it is more convenient to deal with their logarithms ; but this need not frighten us, if we realise that it is only a way of simplifying and shortening the work.

Now the first thing Miss Blagg noticed was that (in terms of our illustration) the rate of interest could not have been .5 per cent. on the average ; it must have been less. If it had been 5 per cent. then we should get about double the sum each time, and the logarithms would successively increase by log. 2, which we have already quoted as .301. The successive increases were certainly not so large ; they could not be put higher than about .240, and probably not so high. Ultimately Miss Blagg fixed on the number .237 as the best she could do for the *average* rate ; and, besides this general departure (from successive doubling) there were individual departures above and below somewhat like those already given in our commercial illustration. When a diagram was made of these departures, they formed a fairly smooth, though wavy, curve, and beyond this there was not much to be said.

But having thus examined the distances of the planets from the Sun she now turned to another system resembling it in some ways but very different in scale ; viz., the system of the satellites of Jupiter, of which there are now eight known. Four of them are large and bright and have been known for 300 years, their discovery being almost the first achievement of the newly invented telescope in 1609—10. But the other four are recent discoveries. The first and innermost (called J. v.) was detected by the sharp eyes of Professor Barnard, using the great Lick telescope, in 1892 ; the next two (J. vi. and J. vii.), which are like a pair of twins revolving at nearly the same distance from Jupiter, were found by Mr. C. D. Perrine on photographs taken with the Crossley reflector at the same observatory in 1904—5, and the last (J. viii.) which is so small and so far from Jupiter that it has been said that the inhabitants of that planet have probably not yet discovered it themselves, was found by Mr. Melotte on photographs taken at Greenwich in 1909. On treating this interesting system in a similar way Miss Blagg was startled to find that the average “rate of interest” came out almost exactly the same as for the Sun and planets ; a coincidence which was the more striking as she had expected a considerable

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difference, and was driven almost reluctantly to the same figures 237. But there was an even greater surprise in store ; she found a remarkable resemblance between the individual departures. In our commercial illustration we tabulated two sets of such departures, one of them just double the other in every item. The similarity found by Miss Blagg is of this kind, though with modifications. The two corresponding curves fit generally, but the individual points do not coincide. Perhaps the best notion of her discovery can now be obtained by inspection of the curve which she published with her paper in the *Monthly Notices* of the Royal Astronomical Society, and which that Society has kindly given me permission to reproduce. It will be seen that not only the systems of the Sun and Jupiter but also those of Saturn and Uranus will all fit on to the same curve. Uranus has only four satellites and it may not mean much that a place can be found for them on a curve with a fair number of sinuosities. But Saturn has nine satellites, and it will probably be admitted that to get all nine on to a previously constructed curve without unduly straining the possibilities is good evidence of some real physical reason behind.

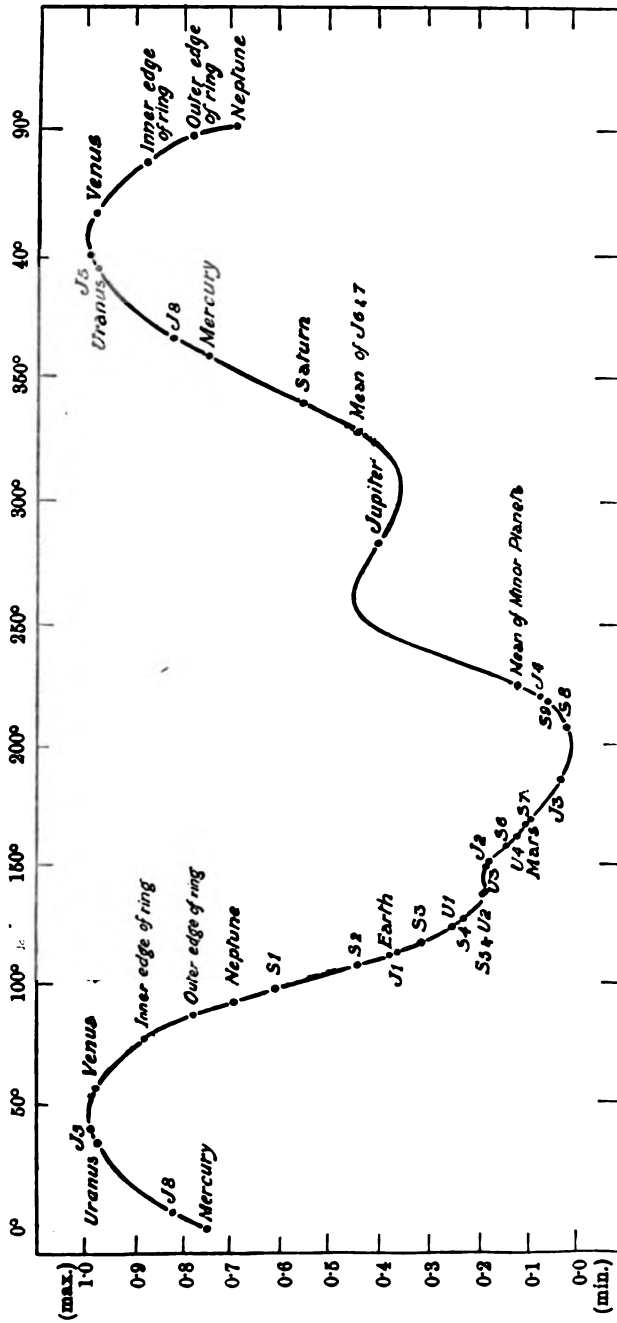
The hint of this physical basis for the curve is, of course, far the most important feature of it ; and though there are matters of detail of considerable interest we will pass them by in order to deal at once with this fundamental question : Assuming for the sake of argument that we can replace Bode's rough " law " by a much more nearly exact law, governing not one system only but several, what would be the significance of this fact ?

To answer the question let us first consider some possible alternatives as to the genesis of such systems. We may have :—

(a) A genesis such as the Nebular Hypothesis sketched in the former article, wherein planets are successively deposited (as rings at first, which ultimately form planets) at the boundary of the contracting atmosphere of a central body. We will further take it as characteristic of this mode that the distances should retain their original values, i.e., the size of the ring thrown off determines the size of the orbit which the planet describes. This is not a necessary consequence, and we will return to the point in a moment ; but for simplicity let us assume it as a formal consequence.

(b) A second method of genesis is exemplified in the case of our

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This curve represents the $f(\theta)$ of the general expression given in the text. Saturn's rings may be treated in several different ways, but they always come into nearly the same place.
The formula makes the slope steeper between 80° and 90° than it is drawn above.

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own Moon, as Sir G. H. Darwin found some thirty years ago. The Moon probably separated from the Earth when the latter was not much larger than it is now, and at first revolved close to the Earth's surface. Owing to tidal action it has gradually receded to its present distance, which is thus very different from the original distance.

(c) A third supposition is that the planets are not children of the Sun at all but aliens "captured" by him in his journey through space.

Now it seems clear that if there is anything in Miss Blagg's suggestion, we may at once rule out any form of supposition (c). Why should casual captives arrange themselves in a definite sequence? There are other difficulties about the "captive" theory which we need not stop to consider; for this one is as sufficient as the thirty-seven wounds which Mark Twain diagnosed as mortal; he said "he did not care about the others."

The recoil from this capture hypothesis sends us almost automatically to the other extreme, hypothesis (a); for on this supposition we have already seen the possibilities of *regular* deposition (in the former article). We may briefly recall that the interval between two depositions was chiefly determined by the time required for a wave of cold to travel from the outer limit of the atmosphere inwards to the central nucleus, thus causing it to contract, and consequently to increase its spin; and finally for this increase of spin to travel outwards again to the periphery of the atmosphere where the ring is to be formed. And reasons were given why the size of the planet thus formed (whether large like Jupiter or small like Mars) might not seriously affect the interval between two crises. On these lines we can see how to explain a regular sequence of the distances established at the outset; and, as already remarked, we must suppose this original regularity to have been maintained.

But this brings us to a difficulty which is somewhat formidable in appearance. Are we entitled to neglect hypothesis (b) completely? In the case of our own Moon it seems certain that tidal action has altered its original distance from its primary enormously; do the moons of Jupiter raise no tides, so that their distances suffer no change of this kind? Startling as it may appear, the answer seems to be actually in this sense—our own Moon has moved

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enormously, Jupiter's moons scarcely at all ; and this is not a new conclusion, but one which startled Sir George Darwin more than thirty years ago. His actual words are as follows :—

“ According to the nebular hypothesis the planets and the satellites are portions detached from contracting nebulous masses. In the following discussion I shall accept that hypothesis in its main outline, and shall examine what modifications are necessitated by the influence of tidal friction.

“ In § 7 it is shown that the reaction of the tides raised in the Sun by the planets must have had a very small influence in changing the dimensions of the planetary orbits round the Sun, compared with the influence of the tides raised in the planets by the Sun.

“ From a consideration of numerical data with regard to the solar system and the planetary sub-systems, it appears improbable that the planetary orbits have been much enlarged by tidal friction since the origin of the several planets. But it is possible that part of the eccentricities of the planetary orbits is due to this cause.

“ We must therefore examine the several planetary sub-systems for the effects of tidal friction.

“ From arguments similar to those advanced with regard to the solar system as a whole, it appears unlikely that the satellites of Mars, Jupiter, and Saturn originated very much nearer the present surfaces of the planets than we now observe them. But the data being insufficient, we cannot feel sure that the alteration in the dimensions of the orbits of these satellites has not been considerable. It remains, however, nearly certain that they cannot have first originated almost in contact with the present surfaces of the planets, in the same way as, in previous papers, has been shown to be probable with regard to the Moon and the Earth.

“ The numerical data in Table II., § 7, exhibit so striking a difference between the terrestrial system and those of the other planets, that, even apart from the considerations adduced in this and previous papers, we should have grounds for believing that the modes of evolution have been considerably different.”—G. H. D., *Phil. Trans.*, Part II., 1881.

The discovery of Miss Blagg is wholly in accordance with this conclusion and seems to entitle us to go further than Sir George Darwin was able to go when he wrote these words. It seems to supply just such data as were “ insufficient ” formerly ; and thus if the suggestion successfully stands the close scrutiny to which it seems to be entitled, and which it will no doubt receive, we may welcome it as furnishing information of fundamental importance not only as to the origin, but as to the whole history of our system.

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We learn not only how the planets were deposited, but the important fact that they must have stayed where they were put.

The consideration of this step forward, (as we may hope it will prove to be) inevitably brings to our minds the past history of this great problem of the relations between planetary distances, which is so intimately bound up with almost all we know of the universe. For it was the study of these relationships with which Kepler started; and it led Kepler to furnish the materials from which Newton proved the law of gravitation. Miss Blagg has returned curiously near to the point from which Kepler set out; his first notion was that of an ideal relationship between planetary distances, which might possibly be exact. He thought he could deduce one from another by the properties of the five possible regular polyhedra (there are five and only five, and it seemed that they must be providentially intended to bridge the five gaps between the six known planets) and his rough draft was astonishingly successful. He hoped to make it perfect if he could only get better measures of the distances; and the search for better measures took him to Tycho Brahe. "Through much tribulation we enter the kingdom"—the well-worn words were never better applied than to Kepler. His supposed treasures turned out to be worthless; he must fling them away, and begin wholly afresh. But he was dauntless; it is not the least noteworthy point in his work that though the distances had burnt his fingers so badly, he was no child to dread the fire. He felt that the truth was to be sought through the study of planetary distance; he sought it with infinite diligence and found it. But his later work never took him again to the neighbourhood of his starting point.

Nearly two centuries later Titius and Bode pointed in that direction, and their indications have been of great value in guiding many investigations. Now Miss Blagg has urged us to approach still nearer; and it certainly seems that we may do so with advantage. But if Kepler's starting point of three centuries ago is really the same as our goal of to-day, it has changed its outward appearance. We can no longer expect to find an ideal relationship rooted in the absolute properties of integers or of geometrical forms; we look instead to the properties of matter.

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By G. Archdall Reid, M.B., F.R.S.E.

IN the last number of *BEDROCK* Mr. H. S. Elliot reviews Loeb's biological essays in a paper full of discussible matter. In two pages he expresses or implies opinions which, found scattered through a number of volumes, have always puzzled me. Bewilderment should be a parent of criticism and a grandparent of understanding. I venture, therefore, to use his article as a peg on which to hang a prayer for enlightenment. Unfortunately criticism, which is as far as I can get at present, tends to bear a hostile aspect. Will he, please, bear in mind, however, that I criticise the original opinions rather than his reproduction of them.

He declares, "Even up to the end of the nineteenth century, the field of heredity continued to be the stamping ground of the rhetorician and metaphysician." Does this statement mean that in biology there were up to the end of the last century no men of science but only rhetoricians and metaphysicians, and afterwards no rhetoricians and metaphysicians but only men of science? Or does it mean that about the year 1900 the rhetoricians and metaphysicians, hitherto the more numerous and noisy, began to succumb to, or to be replaced by, the men of science? If it means neither of these things, then it has little or no real content, and is just a bit of rhetoric. If it means either of them, it is still, demonstrably, no more than rhetoric.

The matter is largely one of definition—of precision of language. Consider the metaphysician first. He need not occupy so much space as the rhetorician. As far as I understand the term, a metaphysician is one who delves in a certain field of thought. It matters not whether he delves well or ill; the mere fact of delving there

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constitutes him a metaphysician. A man of science, on the other hand, works in a particular systematic way in another field of thought. He and his kind constitute a genus in the kingdom non-metaphysician. To a little child a cow, for example, has unquestioned objective reality. The child supposes that the animal exists in space and time and is such in reality as it appears to be. Most men of science, indeed all men of science as such, make this assumption throughout their lives. For them the whole universe has objective reality and is such as it appears to be. As Huxley said, "All physical science starts from certain postulates. One of them is the objective existence of a real world." But to some men comes the thought "I see the cow, I touch it, I hear it, I smell it, and so on. By these means I know of it. But sights, and sounds, and tactile sensations, and the rest are feelings in *me*. They are parts of *my* mind, not parts of the cow. A group of my feelings cannot be like a cow—any more than a pain which I suppose is caused by a knife is like the knife. Far off the cow is a mere dot on the landscape ; near by it has a different appearance ; my feelings have changed, not the cow. The cows I see in my dreams are not real : yet at the time, they seem as real as those that I see in my waking states. How, then, can I trust the stories told by feelings ? Do they tell any truth ? Is there any real cow ? Even if there is a real cow, how is it possible to pass beyond the mere phenomenon and ascertain the nature and origin of the ' thing in itself ' ?" At once we have the metaphysician. He is one who discusses "after physics—things in the abstract, divested of their accidents, relations, and matter." He questions and examines a postulate that science questions not nor examines. Read any book of pure science ; it is obvious that the author has an *unquestioning* belief that the things he describes, plants, planets, men, chemicals, are objective realities and are such as they seem to be. He differs, then, from the metaphysician in that he accepts without question the phenomenal world as real, whereas the metaphysician, as such, seeks to pry deeper.

For all his prying the metaphysician cannot without guessing get beyond the veil of his sense-impressions. If he doubts the objective reality of the cow, he must doubt the objective reality of all other things—his body, his wife, his child, the books he reads, the men

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who wrote them, the world and all that doth inherit it, the whole universe. For all he *knows* his mind is the universe—a lonely thing, perpetually observing phantoms, incomprehensibly suffering from hallucinations. It is futile to argue with him or even to bang his head or crush his toe. He may hop round filling the air with lamentations; but, presently, he will realise that he has no proof that you are more than a very disagreeable phenomenon. Of course, if the metaphysician doubts the existence of bodies, he must doubt the existence of minds other than his own. It is only through bodies that he becomes (or seems to become) aware of other minds. Obviously, therefore, if he would preserve consistency, he should preserve silence. It is folly to explain to other men (mere phenomena, the creations of his own mind) that they have no objective reality and that they must not assume his objective reality.

Up to this point the metaphysician stands on grounds of fact. His feelings are realities, and the only realities certainly known to him. But he is in a *cul-de-sac*, out of which he cannot climb without the aid of mere guessing. All known metaphysicians (all who have expressed themselves) have guessed: hence their divergent and irreconcilable systems of philosophy. (When two people disagree irreconcilably it is certain that the one, or the other, or both are guessing obstinately.) Some have attributed the occurrence of phenomena to God, some to will, some to matter, and some have declared,

“We have no evidence of anything which, not being itself a sensation, is a substratum or hidden cause of sensation; such a substratum is a purely mental creation to which we have no reason to think that there is any corresponding reality exterior to our minds.” *

* J. S. Mill, *Examination of Sir William Hamilton's Philosophy*, 3rd ed. Observe the use of the word “we.” The existence of bodies is doubted or denied, but that of other minds is quietly assumed. As Clifford said: “I believe you are conscious in the same way as I am, and once that is conceded the whole idealist theory falls to pieces.” An excellent example of idealist reasoning is furnished by Professor Karl Pearson in his *Grammar of Science*. Denouncing metaphysics and philosophy as mere guessing, he formulates a purely idealist philosophy which he rhetorically terms science. While he is scornful of an astronomer who assumes the objective existence of stars by asking “Can it be true that these countless orbs are really majestic suns sunk

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If, by means of his guesses, the metaphysician reaches the conclusion that phenomena represent realities, he is on the same plane as the man of science. The one has attained by guessing what the other has assumed without thought. The mathematician, indeed, has tried to be more thorough, but he has not succeeded. If now the metaphysician questions Nature in the orderly systematic way we call scientific inquiry, in which each step is made certain before the next is taken, he is a man of science. It is possible, as in the case of Huxley, to be both metaphysician and man of science. If he continues to guess, he is not a man of science, but he follows nevertheless in the footsteps of a great many people who suppose themselves severely scientific, and who have, apparently, no tincture of metaphysics.

Meanwhile, the point concerning which I desire enlightenment is why Mr. Elliot should so dislike the incursions of metaphysicians

to an appalling depth in the abyss of unfathomable space ? " he assumes the objective existence of the astronomer, and of other men with whom he disputes acrimoniously and to whom he attributes thoughts and emotions like his own. He thinks it scientific to regard objects as phenomena, but discusses the evolution of animals and plants which *ex hypothesi* began millions of years before his mind had being. And so on. Here is a purple patch : " A scientific law is related to the perceptions and conceptions formed by the perceptive and reasoning faculties in man ; it is meaningless except in association with these ; it is the *résumé* or brief *expression* of the relations and sequences of certain of these perceptions and conceptions, and exists only when formulated by man . . . The law of gravitation is not so much the discovery by Newton of a rule guiding the motion of the planets as his invention of a method of briefly describing the sequences of sense-impressions, which we term planetary motion . . . We are thus to understand by a 'law of nature' a *résumé* in mental shorthand, which replaces for us a lengthy description of the sequences among our sense impressions " (*Grammar of Science*, ed. 1900, pp. 86—7). In what intelligible sense can the sense-impression the sun be said to attract the sense-impression the earth ? What are their masses and the squares of their distances ? To me it appears manifest that a law of nature is meaningless unless it describes happenings in a nature which is assumed to have objective reality—to be external to and independent of our minds. " In the scientific sense, a law is a statement of a necessary connection ; it is not something imposed on reality from without, but it is the outcome of the nature of reality itself. The perception of natural laws is due, no doubt, to the synthetic activity of mind, but it is possible only because the connection actually exists in reality. It is because the world is a systematic unity that we are compelled to think it as such. A law, then, in the strict scientific sense, is a fully established statement of universal and necessary connection " (Welton, *Manual of Logic*, Vol. II., p. 200).

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into biology. Whether they be Idealists, Deists, Materialists or any other sort, they cease to be metaphysicians when they enter the domains of science ; they do not discuss metaphysics ; they assume or guess at the existence of reality—if only the reality of other minds. If they are ignorant, or if they continue to guess (if for instance they attribute evolution to a growth-force implanted by the Deity, or if they uphold a vitalist or mechanist philosophy the truth of which they are unable to demonstrate) they can be demolished. Their ignorance or their guessing can be exposed. If Mr. Elliot answers that they cannot be demolished or exposed because a credulous public has faith in false prophets, I can only reply that in that case it is certain that the fault is mainly with the biologists. I am sure that if an ignorant or guessing quondam-metaphysician ventured into other domains of science, for instance, mathematics, physics, astronomy, or chemistry, he would be taken between finger and thumb and eaten like a shrimp.

Now let us turn to the rhetorician. I take it that the antithesis to the rhetorician is not so much the man of science as the logician. The rhetorician, appealing to our emotions, tries to raise up in us, often by means of epithets, an inclination or prejudice, more or less unwarranted, in favour of some opinion. The logician, appealing solely to reason, seeks to demonstrate that, given the facts, a certain opinion must necessarily be true and all contradictory opinions false. All science, that is universally accepted by those who know the facts and are capable of following the reasoning, has been established by means of logical proof. Many splendid examples, for instance, Wells' demonstration of the true origin of dew, and Lord Rayleigh's proof of the existence of Argon, might be quoted.

In all these cases every alternative supposition has been considered and shown to be incorrect. No room has been left for guessing. That is the special feature of each enquiry. That is what is meant when the expression "established science" is used.

The following is the longest and most important paragraph in Mr. Elliot's review. The keynote in it is struck by the words "teleology" and "purpose."

"Whereas it is beyond either my knowledge or my desire to attempt a criticism of the details of Loeb's scientific work, it is

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not without interest to note its relation to the general tendencies of modern biology. Like the Mendelians, Loeb is no believer in the all-sufficiency of Natural Selection. As in their case, his criticism of Natural Selection is founded on objections to the teleological basis of Darwin's great principle. In pre-Darwinian times it was held that every organ, every structure in an animal had immediate reference to some life-conserving purpose; every structure was considered to be developed for some special use: purpose was at the base of morphology. This central conception was in no way changed by the *Origin of Species*. Natural Selection did not question the fundamental assumption of the purposiveness of life and of structure: it merely presented a materialistic explanation as to how that purposiveness was brought about, in place of the spiritualistic, theological or metaphysical explanations previously current. This modification of men's conceptions about the origin of species was, as history has shown, as great a revolution in thought as could possibly be undergone in one generation. But twentieth century biology tends to carry the revolution much further. All the vital movements in modern biology criticise, not so much Natural Selection—for, as Herbert Spencer used to say, Natural Selection is seen to be obviously and inevitably true, the moment the theory is stated—but the assumptions lying at the basis of Darwinism, and received by Darwinism without question from the cloudy superstitions of the past. The modern movements are movements away from teleology. They are movements whose explanations do not include the notion of 'purposiveness': but rest ever more completely on the blind and fortuitous operation of natural forces. Mendelism takes no note of 'purpose' whatever. Loeb's work is scattered throughout with attacks upon the teleological elements in Darwinism: such, for instance, as the very common existence of galvanotropism among animals, which is a reaction alleged to be entirely irrelevant to their needs. And in various other quarters the same tendency may be seen. In the new edition, already half issued, of that great work *Brehms Tierleben*, the position is taken up that the brilliant plumage of humming-birds is due to an incapacity of the kidneys to eliminate the excessively abundant excretory products, caused by their prodigious activity, and that these products accumulating in the feathers produce the bright colours, which (according to the above work) have been found to be of little or no value to the life of the species."*

What is teleology? "It is a branch of metaphysics; the doctrine of final causes and of the uses which every part of nature was designed to subserve; the argument from design in proof of the existence of God." In the days of our grandfathers teleologists supposed that

* BEDROCK, April, 1913, pp. 123—4.

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a conscious being had, of set purpose, designed the structures of plants and animals for the gratification or the affliction of mankind. At the present time, in view of the admitted fact of evolution, they suppose that this conscious being designed the structures of plants and animals to perform the functions they do perform. The essence of teleology is the notion of conscious design antecedent to execution. We make a teleological statement when we attribute the expansion of freezing water to the wisdom of the Deity. We do not make a teleological statement when we indicate the effects of wave selection—pebbles gathered in one place, sand in another, weed in a third.

Darwin observed that offspring varied from their parents, some being superior, some inferior (*i.e.*, better or worse fitted for the struggle for existence). This *fact* may be verified by anyone at the present day. Darwin also supposed that, on the average, the fittest survived and continued the race while the unfittest perished without offspring. Anyone may not only suppose but observe this *fact*, for instance in the case of human races afflicted by diseases. He supposed further that from this unconscious natural selection of living beings evolution resulted and species originated. Running all through Darwin's thinking is the assumption that the structures of living beings are useful to them—useful to the individual in his struggle for existence, or in his struggle for offspring. He did not attempt however to prove the general utility of structures—to prove the utility of limbs, and lungs, and hearts, and bowels, and organs of generation, and the like. I think he must have had some idea that people of ordinary intelligence would perceive the *fact*. Undoubtedly he, himself, regarded living beings with their complex and exquisitely adjusted interlocking parts, their wonderful chemistry, and their amazing fitness to their special environments, as adaptional forms.

This is the theory that is said by those austere thinkers the exponents of "modern science" to be based on assumptions received without question from "the cloudy speculations of the past." I am glad Mr. Elliot adds: "I am not here expressing any opinion on these views; I am only noting tendencies." I am able to write with greater freedom than would otherwise have been possible. The tendencies certainly exist and it is time they were examined.

Very obviously there is no hint of teleology—of conscious design

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antecedent to execution—in Darwin's theory. It is a matter of history that when that theory was first published it met with execration just because it tended to destroy current teleological notions. That dislike for teleology which especially distinguishes biologists as compared with men in other departments of science had origin in teleological opposition to a supposition which, as they became convinced, deserved to be considered on its merits. At the present day "modern science" is hailed with delight by teleologists because it is thought to have discredited Darwinism. Hence the absurd pulpit statements that there is now an approximation between religion and "true science." Professor Bateson himself has declared :—

"I see no ground whatever for holding such a view, but in fairness the possibility [of orthogenesis] should not be forgotten, and in the light of modern research it scarcely looks so absurdly improbable as before."*

It would seem, then, that Mendelism, so far from taking no note of "purpose," keeps an eye on that possibly useful retreat.

One thing ought to be made very plain. Neither Loeb, nor the Mendelians, nor the Mutationists have demonstrated, or even attempted to demonstrate, that Darwinism has a teleological basis. The statement that it has such a basis (or even implications) has been, whenever made, no more than a mere statement unsupported by evidence or any sort of proof, a disingenuous appeal to the prejudices of biologists, an item of rhetoric dishonestly intended to exalt one supposition by discrediting another, a product of what is perilously akin to charlatanism. At best, and I think most often, it has resulted from extravagant incapacity to think clearly and to use words correctly. I am aware that language such as this is inexcusable unless it is entirely accurate—unless it is not in the least rhetorical, and that I shall be convicted of a grave offence if it can be shown that Darwinism, as propounded by its author or as commonly accepted, has teleological implications. I do not think, however, that anyone will attempt to correct me.

Meanwhile it is time to protest against the frequent use of

* Bateson, *Heredity and Variation in Modern Lights, Darwin and Modern Science*, p. 101.

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such epithets as "teleological," "metaphysical," "pre-Baconian," "deductive," and the like in certain classes of twentieth century biological literature. If the passages in which they occur be examined it will be found that they are nearly always used rhetorically and inaccurately to create prejudice in readers or conceal it in writers, and that they usually precede cloudy speculations, illegitimate pretensions, or actual mis-statements—such cloudy speculations as that the brilliant plumage of humming-birds is due to superabundant kidney excretion, such illegitimate pretensions as that Loeb's experiments on the eggs of sea-urchins have definitely reclaimed the field of heredity for physical chemistry, such actual mis-statements as that experiment, or biometry, or the collection of family histories are our sole means of ascertaining truth in this field. They are not used in other sciences and would not be tolerated there. Imagine the attitude of an astronomer or chemist were an attempt made to influence him against a supposition by means of them. Science is not concerned with whether a statement is teleological or anything of the sort. It is concerned only with the truth and relevancy of statements.*

The opposition to Darwinism, then, is both teleological and antiteleological. Darwinism, like teleology, seeks to account for the occurrence of useful structures. While, therefore, the teleologist perceives in it the negation of some of his most cherished "proofs," the antiteleologist (who in this case is always an adherent of "modern science") discovers between it and the object of his aversion the same fatal identity that like-minded people perceive between four o'clock and four pounds of butter.

But some of the opposition to Darwinism is merely non-teleological; and, since this opposition is founded on the allegation that this or that contradictory supposition is more in accord with the known facts, it is at least professedly scientific. At first the

* I remember my first meeting in science with "metaphysical." I was a new student and the occasion was typical of much future experience. My Scotch professor of surgery made contradictory statements in the same lecture. I sought enlightenment. At first he looked puzzled, then annoyed, then his countenance cleared and he announced I was "metaphysical." That ended the matter for him, and for me my faith in him. I am told he was a good surgeon; I am sure he was a confused and occasionally a dishonest thinker.

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Lamarckian hypothesis held the field. It has now very few adherents and its place is taken by Mendelo-Mutationism.

The Mendelo-Mutationist hypothesis is not very easy to define, for there are several varieties of it. It is correspondingly difficult to deal with its opposition to Darwinism. Sometimes, apparently, it is accepted because it is a product of "modern science and modern methods," whereas Darwinism is rejected because it is a product of the cloudy speculations of the past, or of essay-writing, or rhetoric, or metaphysics, or something of the sort. Sometimes Darwinism is totally rejected, as in the following passage:—

"With faith in evolution unshaken—if indeed the word faith can be used in application to that which is certain—we look on the manner and causation of adapted differentiation as still wholly mysterious."*

Sometimes "Natural Selection is seen to be obviously and inevitably true, the moment the theory is stated" and Darwinism is rejected only in part, as in the following passages:—

"It is, therefore, reasonable to regard the mutation as the main, if not the only basis of evolution. . . . The new character that arises as a mutation has its representation in the gamete. Once it has arisen selection alone can eliminate it . . . The small fluctuating variations are not the materials on which selection works."†

"The claim of the opponents of the theory that Darwinism has become a dogma contains more truth than the nominal followers of this school find pleasant to hear; but let us not, therefore, too hastily conclude that Darwin's theory is without value in relation to one side of the problem of adaptation; for, while we can profitably reject, as I believe, much of the theory of Natural Selection, and more especially that adaptations have arisen because of their usefulness, yet the fact that living beings must be adapted more or less to their environments in order to remain in existence may, after all, account for the widespread occurrence of adaptation in animals and plants."‡

"To imagine that a particular organ is useful to its possessor, and to account for its origin because of the imagined benefit conferred, is the general procedure of the followers of this school."§

Here then we have three varieties of the opposition offered by modern science" to Darwinism. Professor Punnett, accepting

* Bateson, *Darwinism and Modern Science*, p. 99.

† Punnett, *Mendelism*, 2nd ed., pp. 72—3.

‡ T. H. Morgan, *Evolution and Adaptations*, p. ix.

§ *Op. cit.*, p. 435.

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Natural Selection as the cause of adaptation, differs from the ordinary Darwinian only in so far as he supposes that Nature selects mutations (which "selection alone can eliminate") as the materials with which she works. Professor Morgan, going further, declares that organs have not arisen through Natural Selection, but have only been preserved by it—that is, that organs have not *arrived* through Natural Selection, but only *survived* through it. Professor Bateson, going to extremes, is complete in his rejection. He will not admit that Natural Selection is concerned either with the arrival or the survival of adaptations. He is as scornful as Professor Morgan:—

"But given variations—and it is given: assuming further, that variations are not guided into paths of adaptation—and both to the Darwinian and to the modern school this hypothesis appears to be sound if unproven—an evolution of species proceeding by definite steps is more, rather than less, easy to imagine than an evolution proceeding by the accumulation of indefinite and insensible steps. Those who have lost themselves in contemplating the miracles of Adaptation (whether real or spurious) have not unnaturally fixed their hopes rather on the indefinite than on the definite changes. The reasons are obvious. By suggesting that the steps by which an adaptive mechanism arose were indefinite and insensible, all further trouble is spared. While it could be said that species arise by an insensible and imperceptible process of variation, there was clearly no use in tiring ourselves by trying to perceive that process. This labour-saving counsel found great favour. All that had to be done to develop the evolution-theory was to discover the good in everything, a task which, in the complete absence of any control or test whereby to check the truth of the discovery, is not very onerous. The doctrine '*que tout est au mieux*' was preached with fresh vigour and examples of that illuminating principle were discovered with a facility that Pangloss himself might have envied, till at last even the spectators wearied of such dazzling performances."*

Before dealing with the varieties of the "modern scientific" opposition to Darwinism it will be well to consider this very fundamental question of adaptation. The ordinary man perceives a great many adaptations in the human being—eyes, mouth, ears, limbs, and the like. He even knows, when he has reached puberty, the function of such tufts of hair as eyebrows and lashes, and such

* *Darwinism and Modern Science*, pp. 99—100.

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patches of colour as occur on the surface of eyes and cheeks. He perceives so many adaptations and so little that is non-adaptive that if a dissected body were shown him and he were asked, "What is the function of this organ, and this, and this other? Have they any use?" he would probably answer: "I don't know. I suppose they have their uses." The attitude of the ordinary man was that of the human physiologists of past times. Possessed with the idea that organs had useful functions, they embarked on a sea of investigation, with the result that now the modern physiologist can point to hardly a structure (or a tissue, or an acid, or a salt, or a ferment) in the whole body which is not known to be useful, or which, if vestigial, was not formerly useful. He realises that (apart from variations) not only are individual structures adaptations, but also, what is even more arresting and suggestive, that these adaptations are co-adapted—that the human body is a vast and almost inconceivably complex and finely adjusted combination of machine and chemical factory. Modern human physiology owes its existence to the fact that its students "lost themselves in contemplating the miracles of Adaptation."

The functions of the structures of plants and lower animals, especially those far removed from man, are less well known. Comparatively speaking, not only has much less study been devoted to them, but, for obvious reasons, the task is more difficult. Nevertheless, plant and animal physiologists proceed on the same assumption as the human physiologist. They suppose that the structures they investigate have useful functions and they endeavour to discover them. This, precisely, is the attitude which, in Darwinians, is condemned by the modern scientist. Says he, for example, "This spot on the butterfly's wing, this tuft of hair on the turkey's breast, this brilliant plumage of the humming bird; do you *know* that they have useful functions? Can you prove that they arouse sexual emotions in the female? No, you cannot. According to the general procedure of your school you are imagining that a particular organ is useful to its possessor and accounting for its origin because of the imagined benefits conferred." So might a severely sceptical physicist argue, "It is true that stones fall in England and Brazil and South Africa. But that is no proof that they fall everywhere—on the top of Mount Everest, for example."

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From the fact that the function of a structure is unknown, it is but a step to the assumption that it—that innumerable structures—have no useful functions, and that animals that live and move and have their being in a world full of dangers are, on the whole, compounded of useless parts. It is vain to reply “Let us leave such things as spots on butterflies’ wings and discuss familiar structures, or such a familiar animal as man, concerning which you and I can have positive knowledge.” The modern scientist will have nothing to do with things so unscientific, so popular, so indisputably known as these.

It has not been proved that a single structure in any plant or animal is useless. Experience, physiological and other, proves that the degree of our incapacity to perceive the utility of structures is always directly proportionate to the extent of our ignorance and incapacity to think clearly. But even if it were proved that many structures are, and have always been, useless, it would not do away with the fact that there has been adaptation—with the known fact that individuals survive and species persist because they are fitted to their special environments by virtue of useful structures which, at any rate, immensely preponderate over useless structures, with the known fact that evolution has been, in essence, adaptation. It is no more unscientific to attempt to account for this quite certain fact than for any other—for instance, the waxing and waning of the moon, or its roundness, or its lack of radiant heat. How does it happen, then, that Mendelo-Mutationists are so ostentatiously contemptuous? I take it that here we have merely an example of rhetoric. Adaptation constitutes a principal difficulty in the path of “modern science.” The rhetoric appears to have achieved considerable success. There is now quite a considerable sect of Mendelo-Mutationists. However, this matter will be more fully discussed in the last section of the present article.

Mendelo-Mutationism is not a theory of evolution, if by evolution we understand adaptation. It can be used in conjunction with a theory of adaptation; but, by itself, it is merely a theory of inheritance. Only a limited number of theories of adaptation have been conceived, or, apparently, can be conceived—the theory of special creations, the theory (also teleological) of an adaptive growth-force, the Lamarckian theory, and the Darwinian theory. All others are

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merely modifications of these four. Mutationism must ally itself with one of them or it cannot account for the fact that plants and animals are adaptional forms able to persist in a hostile world.

Professor Bateson rejects every conceived theory of evolution. For him the fact that adaptation exists and seemingly has always existed is a complete mystery. As far as I am able to judge he perceives that his theory of heredity is incompatible with any conceivable theory of adaptation, and as a consequence he rejects every theory that has been conceived and even declines to "lose" himself "in contemplating the miracles of Adaptation." His attention has been called to the fact that whenever we are able to observe closely (as among human beings in relation to their diseases), selection of fluctuations plainly and invariably occurs and leads to evolution. But, since he has already decided that fluctuations "are impalpable" * and, therefore, beyond Nature's powers of selection, the matter, for him, has ended.†

Professor Morgan supposes that selection has been concerned with the survival, but not with the arrival of organs, which arose by mutation. Well, take the case of a very well-known and highly adaptive organ, the human fore-limb. Is it believable that it appeared, as his words imply, in the progeny of ancestors that had no fore-limb? No. Then it did not arrive through mutation, it must have adaptionally modified from a fore-limb that was

* Mendel's *Principles of Heredity*, p. 3.

† Mutations are sometimes described as "sports" and "differences of wide amplitude," while fluctuations are described as "impalpable." But very plainly these descriptions are incorrect. Some mutations are small and some fluctuations large (see Punnett, *Mendelism*, p. 72). Thus, though differences of size and strength between individuals of the same variety (e.g., human) are often considerable, they are, as a rule, fluctuations. A great deal of merely rhetorical contempt has been poured on the notion that Nature is incapable of selecting "impalpable" differences. It is well, however, to examine the actual facts. A man who perishes of tuberculosis in spite of every care does not differ impalpably from one who lives unharmed in the midst of infection. Nor is the difference between a very tall, muscular, or hardy man and his opposite impalpable. Suppose that stature is the character Nature is selecting. Imagine all the individuals of a variety, arranged according to height, standing in a row. Nature would especially preserve those at one end, and especially eliminate those at the other. She would contrast extremes. So the average of the race would be raised. The rhetorician endeavours to fix our attention on contiguous individuals and then exclaims that the differences are impalpable.

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different. Experience shows that even mutations are not always adaptive ; indeed, the occurrence of a clearly adaptive mutation has not yet been recorded. What, in that case guided the modification ? What made it adaptive ? What selected the right variations that went towards the making of the human fore-limb ? Was it a divinely directed growth-force ? Was it Lamarckian transmission ? Was it Darwinian Selection ? Professor Morgan must choose or like Professor Bateson remain mystified. Mendelo-Mutationism alone will not serve his purpose. If he declares that by "organ " he did not mean anything so large and complex as a limb, let him take a detail—a group of muscles, one muscle, a part of a muscle, a nail, anything. The same reasoning applies. Every detail is made up of smaller details. All are useful modifications of structures that, under somewhat different conditions, were as useful in the ancestry. What selected the variations so that only those that were adaptive persisted ? I take it Professor Morgan's contempt is either based on a very profound confusion of thought, or is purely rhetorical.

Professor Punnett definitely accepts Darwinism with the reservation that mutations, not fluctuations, are the variations which Nature selects. According to him "fluctuations are often due to conditions of the environment, to nutrition, correlation of organs, and the like. There is no indisputable evidence that they can be worked up and fixed as a specific character." As we have seen, he regards—on the strength of experiments extending over half-a-dozen generations—mutations as eternal unless eliminated by selection. Now consider race-horses, which are typical examples, oft-quoted by Mendelo-Mutationists, of evolution founded on the selection of fluctuations—of evolution which is not eternal but tends to retrogress on the withdrawal of selection. Curiously enough, race-horses furnish almost the only instance of varietal change quoted by them which at all resembles, in that it is adaptive, the changes that actually occur in Nature. If a pair of race-horses be reared in the environment of dray-horses and mated, the offspring are race-horses. They may not turn out good examples of their kind, but, on the average they will be as good as the progeny of race-horses reared in the normal environment. Similarly, you cannot get race-horses out of dray-horses by changing the environ-

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ment. Clearly the difference here is germinal. Yet Professor Punnett says that fluctuations are often due to conditions of the environment, whereas mutations have their representatives in the germ. I have wondered occasionally why the word "often" was used by him. Darwinians insist, of course, that variations and acquirements are sharply distinct, and that fluctuations are variations. Does Professor Punnett seek, gently and rhetorically, to urge his readers to the end reached by that brave theoriser Professor Castle, who states positively that all fluctuations are of the nature of acquirements ?*

All authorities who have written on scientific method are agreed that no hypothesis deserves attention unless it can be tested and unless it can be conceived as true. Hypotheses are tested and it is ascertained whether they can be conceived as true by tracing their consequences.

"The sole condition to which we need conform in framing any hypothesis is that we both have and exercise the power of inferring deductively from the hypothesis to the particular results, which are to be compared to known facts."†

The consequences of the Mendelo-Mutation theory are not traced by noting mutations and supposing that they are the materials on which Nature works. But they are traced if we try to imagine what would be the effect of a mutation occurring within that closely adjusted machine and chemical factory the living body. If Professor Punnett will explain how, under such conditions, a mutation could be favourable and then mention mutations which have been favourable, he would eliminate much of the opposition now encountered from men who have a weakness for suppositions that are at least believable.

The Mendelo-Mutation hypothesis would be rendered still more believable if the following queries were answered. I have asked them before and have always failed to get a reply. I live in hope, however. I think, indeed, I shall one day force an answer. Either Mendelo-Mutationists will prove that I am wrong, or they will admit that they have been guessing when means of proving lay at hand.

* "The Mutation Theory of Organic Evolution, from the Standpoint of Animal Breeding," *Science*, April 7th, 1907.

† Jevons' *Principles of Science*, p. 265.

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(1) It is admitted that artificial varieties have differentiated largely through the selection of mutations. Man is forced to choose fluctuations when he seeks to bring about the evolution of such a character as speed (*e.g.*, in horses or dogs), in the production of which many co-adapted parts are concerned, but more often he

“begins his selection by some half-monstrous form, or at least by some modification prominent enough to catch the eye or to be plainly useful to him.”*

It is admitted also that the reproduction of mutations is Mendelian—that offspring and descendants reproduce one *or* other of the alternative characters, not a blend of both. The Mendelian hypothesis is that in the “impure dominant” there is patency of one unit and latency of the other, but that segregation of units subsequently occurs, so that in the “pure” descendants there is no latent unit. The rival supposition is that there is no such separation, but that that latency of one unit which occurs in the “impure” dominant is continued in the so-called “pure” descendants. Trace, now, the consequences of the rival hypotheses. If segregation really occurs, then, when an individual who has mutated is crossed with the ancestral form, there should be no reappearance of the ancestral form among the offspring of those “pure” descendants that reproduce the mutation. On the other hand, if segregation does not really occur, if there is no real “purity,” if only latency of the apparently absent unit occurs, then these so-called “pure” descendants should sometimes reproduce the ancestral form. For instance, a mutation in colour sometimes occurs among grey rabbits; an albino or black individual is born. If this mutant be crossed with a grey rabbit and the descendants inbred, some strains of them produce (for at least some generations) only grey individuals, and others only white (or black, as the case may be) individuals. These are the strains that are thought by Mendelians to be “pure.” But if white (from which grey is supposed to have segregated) be crossed with black (from which it is also supposed to be absent) grey at once reappears. Scores of similar cases may be cited from the literature of domesticated varieties of all kinds. Whence the grey if it had segregated, if it was not merely latent? Cuènot, resolutely piling one guess upon another in true pre-Baconian

* Darwin.

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fashion, has supposed that colour (and other characters) depends on the co-operation of two factors, and that the factors for grey got separated in the white and black rabbits, the one going to the one type and the other to the other. But, even if we admit this shocking guessing "to a scientific status," how shall we account for the fact that *purely* bred offspring have in hundreds of instances reproduced ancestral traits that are supposed to have departed utterly when they became "pure?"

"Thus we see that, in purely bred races of every kind known in Europe, blue birds occasionally appear having all the marks which characterise *C. livia*." *

Bewildered Mendelians themselves have recorded cases in which *pure* dominants have produced recessives, and recessives dominants. In some cases the same individual has reproduced first the traits of the variety whence it was derived, and afterwards the long lost ancestral traits.† Whence in such instances these ancestral traits? Here there can have been no reunion of divorced factors. If, after contemplating such instances, Mendelians can still conceive the doctrine of segregation as true, I can only suppose that they belong to a very faithful but not very logical type—the type of the religious sectarian. It must be borne in mind that the evidence that "pure extracted" descendants are really impure is precisely of the same character as that which leads Mendelians to conclude that "impure dominants" are impure. In each case it is the becoming patent of antecedently latent traits.‡

(2) According to Mendelo-Mutationist theory—

"The facts . . . leave no room for doubt that at least one character of each pair of simple allelomorphs has arisen discontinuously. The fact that the gametes of the cross transmit each member of the pair pure is as strong an indication as can be desired of the discontinuity between them." §

* Darwin, *Animals and Plants*, Vol. I., p. 206.

† *Ibid.*, Vol. II., p. 29.

‡ I know of one attempt, and one only, to explain away these facts. "The explanation is easy," it was said. "Here we have instances of compound allelomorphs—compounds in which one element of the combination is patent and the other latent." This answer amounts, of course, to a complete admission of the truth of the supposition that is supposed to be controverted. The latter claims nothing more than that one element is patent and the other latent.

§ Bateson, *First Report to the Evolution Committee of the Royal Society*, p. 151.

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Here the claim is that Mendelian reproduction of a character is proof of its origin by mutation. Domesticated varieties afford abundant testimony that the claim is, at least in large measure, valid. It is actually a fact that the mutations of parents are reproduced by offspring and descendants almost unblended with their alternatives. * Indeed, Mendelian reproduction is the distinguishing mark of a mutation. *But, if Mendelian reproduction indicates origin by mutation, the converse must also be true; blending of parental unlikenesses in the offspring and descendants must indicate origin by fluctuations.* Now, while the varieties that man has created (especially the "fancy" breeds) transmit, when crossed, their distinguishing peculiarities unblended, *the varieties that Nature has evolved display, when crossed, blended inheritance as a very general rule.* Man is a conspicuous example; and among men the mulatto and the Eurasian are well-known instances. In nearly all characters (eye-colour being the only exception) the blending is unmistakable and perfect. And in all cases it is permanent. Thus, mulattoes bred together for any number of generations reproduce only mulattoes; never pure whites and pure blacks. If more white blood is introduced "the touch of the tar brush" grows fainter with each infusion. An interesting example of the blending which occurs when lower animals of natural varieties are crossed was lately, and, for all I know, is now, to be seen in the London Zoo—brown bear crossed by polar bear and the hybrid recrossed by polar bear. Yet more; as we have seen, when artificial varieties are crossed, and sometimes in the absence of crossing, long latent ancestral traits reappear in abundance; but when natural varieties are crossed their reappearance is, to say the least, exceedingly rare.† Here, then, is

* Almost, but not quite. "Very frequently, if not always, the character that has once been crossed has been affected by its opposite with which it was mated and whose place it has taken in the hybrid. It may be extracted therefrom, to use in a new combination, but it will be found altered. This we have seen to be true for almost every character sufficiently studied . . . Everywhere unit characters are changed by hybridizing" (Davenport, *Inheritance in Poultry*, p. 80). I cannot conceive how unit characters can be changed unless there is blending of allelomorphs or unless the alternative allelomorph is present in an imperfectly latent condition.

† Compare *Oenothera lamarckiana*, a garden plant, the reappearing ancestral traits of which de Vries mistook for fresh mutations (hence his surprising hypothesis of ever-mutating species), "with the hundreds of species that grow

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evidence, world-wide in volume and much of it experimental—just the kind of evidence Mendelians value. Does it leave us with any hypothesis conceivable as true except that Artificial and Natural Selection differ sharply, and that the difference consists in that the former uses for its materials mutations, while the latter uses fluctuations ?

(3) The problem of sex, the problem as to why, in so many cases, two germ cells unite to form a single individual, is important. But it is not the only, or even the most important, problem presented by Nature to the student of heredity. The problem as to how and why sex was evolved is also important ; but it is not the only problem presented to the student of evolution. There are other problems, questions concerning variation, adaptation, structures (*e.g.*, brain) and functions (*e.g.*, that of mind), unconnected with sex, which are at least as important. In enquiries concerning these other problems sex introduces a complication, irrelevant matter, an element of confusion. Were it possible to gather sufficient data, such problems could be as well studied in parthenogenetic species. Mendelians are under the impression that they are studying the whole subject of heredity. But I should like to ask Professor Punnett if he can name a single problem, other than that of sex, which is being studied ? As far as I am able to perceive, Mendelians are merely occupied in ascertaining the results that flow from the crossing of unlike types. The conclusion they have reached is that Nature has so managed that, when two unlike individuals (who, since they have reached puberty, are both adapted to the environment) conjugate, the unlike structures are mingled in haphazard confusion in the unfortunate offspring. They suppose Nature to resemble a workman who separates, and mingles, and then unites at haphazard the parts of two unlike machines.

Leaving now this branch of our subject, consider the following passages :—

“ No one can survey the work of recent years without perceiving that evolutionary orthodoxy * developed too fast, and that a great deal has got to come down ; but this satisfaction at least remains

wild in Holland,” with which he also experimented, and in which “ no real mutability could be found.”

* That is the belief that evolution is in essence adaptation.

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that in the experimental methods which Mendel inaugurated, we have means of reaching, certainly in regard to the physiology of Heredity and Variation upon which a more lasting structure may be built.”*

“The recognition that only by experimental methods can we hope to place the study of zoology on a footing with the science of chemistry and physics is a comparatively new conception, and one that is by no means admitted as yet by zoologists. I do not wish to disparage those studies that deal with the descriptive and the historical problems of biology. They also offer a wide field for activity, and the more familiar we become with the structure and modes of development of animals, so much the better can we apply the experimental method. In fact, many of the problems of biology only become known to us as the result of direct observation. The wider, therefore, our general information, the greater the opportunity for experimentation.

“It is undoubtedly true that many zoologists who have spent their lives in acquiring a broad knowledge of the facts of their science fail to make use of their information by testing the very problems that their work suggests. This is owing, no doubt, to their exclusive interest in the observational and descriptive sides of biology ; but also in part, I think, to the fact that the experimental method has not been sufficiently recognised by zoologists as the most important tool of research that scientists employ.” †

“The essence of the experimental method consists in requiring that every suggestion (or hypothesis) be put to the test of experiment before it is admitted to a scientific status. From this point of view the value of a hypothesis is to be judged, not by its plausibility, but by whether it meets the test of experiment.” ‡

“It is sometimes said that Nature has already carried out innumerable and wonderful experiments, and that we can never hope to excel her in this power. Is it not better, therefore, to examine patiently and reverently what she has done, and in this way learn how her processes have been carried out ? Let us not be blinded by rhetorical questions of this kind. No doubt Nature has carried out prodigious experiments ; but we can never be certain how she has obtained her results until we repeat the process ourselves. What would the chemist or the physicist say if he were told that Nature has already carried out experiments on a much larger scale than he can hope to accomplish, and that he should drop his experimental methods and study his physics in a thunder-storm and his chemistry in an eruption.” §

“If the truth must be told, the experimental method was given

* Bateson, *Darwin and Modern Science*, p. 100.

† T. H. Morgan, *Experimental Zoology*, p. 3.

‡ *Op. cit.*, p. 6.

§ *Op. cit.*, p. 8.

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up for a long time by the majority of specialists themselves in favour of the controversial, and, indeed, this tendency has by no means yet died out among the habits of some professed evolutionists. On the other hand, during the past fifteen to twenty years, a few scattered workers have diligently applied themselves to the study of the facts of variation and inheritance, with results which already more than justify the anticipation in which their work was begun—namely, that by such methods alone can any real progress in our knowledge of the processes of evolution be brought about.”*

Now, either these passages, and many others of similar purport and quality, were written in elementary ignorance of the nature, history, materials, and methods of science, or they are no more than rhetoric. The assumption that experiment is especially accurate or scientific is purely nonsensical. Experiment is merely one of the several methods of observing that are used in science, and as such is no more accurate or scientific than any other method. Like every other method of observing it is especially accurate, and indeed can be used, only on particular occasions. Some facts are patent to our senses and, if they are to be known at all, must be simply observed—for example, the facts that water is liquid, that men have heads, that they resemble apes more than other living beings, that offspring both resemble and vary from their progenitors, and so on, with respect to millions of facts. Other facts, for example that water is compounded of oxygen and hydrogen, or the function of the thymus gland, are so obscured by the conditions in which they occur that they cannot be observed, or inferred with any degree of certainty, unless the obscuring conditions are removed. An experiment is an attempt to remove the obscuring conditions. It is useful only when there are obscuring conditions. The most successful experiment does no more than make a fact which was previously obscured as patent as one that was open to simple observation from the first. Physics and chemistry are accurate and experimental, but only uncommon confusion of thought can attribute the former quality to the latter. They are accurate because their students both had and used the power of measuring the facts minutely and testing the thinking founded on the facts carefully and thoroughly. They are experimental because the facts could be observed in no other way. Physicists and chemists

* R. H. Lock, *Variations, Heredity, and Evolution*, p. 3.

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would not have been so absurd as to waste time and labour over experiment if their data could have been simply observed. Mathematics and astronomy are very accurate but not experimental. It is axiomatic that all facts, no matter how observed, are equal before science.

The statement that the study of heredity and evolution must be experimental implies one of two things—that it is not possible to observe relevant and authentic facts in any other way, or that we must limit our attention and draw our conclusions from only a part of the available evidence. In either case the statement is conspicuously untenable. The facts derived from simple observation are very numerous. With the exception of physiology, every science—zoology, botany, embryology, anatomy, palæontology, for example—which is concerned with life is founded exclusively, or almost exclusively, on them. In the case of any given fact, we cannot tell, till we have examined and thought about it, whether we can, or cannot, make use of it in our studies. Such men as Darwin and Weismann have made immense use of direct observation. For instance, experiment is hardly mentioned in that greatest of biological works, the *Origin of Species*.

Facts may be used to found suppositions on, or to test suppositions by. The same facts cannot, in the case of any given supposition, be used for both purposes. Professor Morgan declares that every hypothesis must be put to the test of experiment before it can be admitted to scientific status. Anyone, reading him, would suppose that such testing is peculiarly decisive and that it has been used in "modern science." Really it is no more decisive than any other testing (i.e., testing by means of facts collected in other ways), and—I am weighing my words very carefully—it has never, not even in a single instance, been employed by Professor Morgan and his school. The whole of Mendelo-Mutationist literature may be examined and no case will be found in which facts have been used in any other way than as foundations for suppositions. Not only has every hypothesis been left as a mere supposition (a guess), but actually no *attempt* has been made to establish it. I am dealing here with a very simple matter. If I am wrong a demonstration of the error should be most easy and most disastrously effective. I have no fear, however.

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We have seen what the Mendelo-Mutationist facts are—on the one hand the occasional occurrence of mutations and on the other the alternative reproduction of mutations. We have seen what the Mendelo-Mutationist suppositions are—that allelomorphs segregate and that evolution is founded on mutations. We have seen what the rival suppositions are—that allelomorphs do not segregate and that evolution is founded on fluctuations. We have seen what testing a supposition implies—a demonstration that it accords with all the relevant facts and that no rival supposition does. A supposition is said to have been experimentally tested when the facts by which it is proved (and rival hypotheses disproved) have been revealed by experiments devised for that purpose. The Mendelo-Mutationist facts obviously do not prove the Mendelo-Mutationist suppositions; experiments have not, and apparently cannot, be devised which would prove them; the rejection, unjustified except by such curious passages as I have quoted, of facts derived from other sources prevents all attempts at proving. Professor Morgan's statement, therefore, is merely rhetorical. It is intended to lead his readers to believe that suppositions, which are no more than guesses founded on facts experimentally discovered, are theories which have been established by experimental tests. I daresay he shares the delusion; but that is hardly even an excuse.

I think it is sure to be said that I decry experiment. Of course I do no such thing. Unquestionably the method is an admirable and convenient means of testing when it can be applied. But it is not always applicable and a good thing is not improved by exaggerated claims. You do not, for example, prove that a man is a thief more decisively by the experiment of turning out his pockets and finding your spoons there than you do by merely observing them sticking out of his pockets. Beautiful examples of experimental *testing* abound in the history of physics, chemistry, and physiology. But for it, these sciences would be non-existent. Here, however, laudation of the method has not been substituted for the use of it.

It is true, then, that "the field of heredity has continued to be the stamping ground for the rhetorician." Moreover, as Mr. Elliot notes, "journalists, sciologists, demagogues, and popular lecturers

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at large have been stampeding all over the country." Other sciences are not similarly afflicted. Compare the present unhappy position of biology with that of chemistry and astronomy, which won their battles against far more fearful opponents more than four centuries ago. The question as to why biology should be so peculiarly hag-ridden is fundamentally important. The best antecedent to attempts at cure is a knowledge of the causes of evil. I venture with the humility, diffidence, and respect for established authority, especially "scientific" authority, which has characterised the present article, to suggest an answer. If there is some repetition of matter already published in *BEDROCK*, the necessity for knitting together the facts and reasoning may serve as an excuse. Moreover, in these days of so much vague chatter concerning "modern methods" and "modern science," the real method of science cannot be too strongly insisted on or too often hammered in.

There are two kinds of science, one of which is usually termed descriptive and the other interpretative. We describe when we limit ourselves to telling of the characteristics and behaviours of objects, when we state, and do no more than state, resemblances, differences, co-existences, and sequences. Thus we describe when we state that a man is a mammalian vertebrate who passes by such and such stages from germ to old age. We then indicate merely that he resembles certain other animals in that he has a backbone with which breasts co-exist, and that a certain series of events occurs in a certain order.

We interpret or explain when we trace cause and effect, when we link antecedents with consequents. Thus we interpret when we try to account for the existence of breast and backbone in man and for his passage from germ to old age. In the case of a series of events, we describe when we state *how* things happened : we interpret when we explain *why* things happened. Notwithstanding recent publications there is a vast and real difference between how and why. The similarity which exists between them is like that which exists between four pounds of butter and four o'clock. For example, if we asked a surgeon how he did an operation he would tell us of the steps of it ; if we asked why he did it he would name an antecedent, for instance, a tumour. Description, expressed or implied,

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must precede interpretation : for we cannot reason about things unless we have some idea of what they are like.

Some sciences (*e.g.*, systematic botany) are mainly descriptive ; others (*e.g.*, mathematics, physics) mainly interpretative. A training in descriptive science tends especially to endow the learner with a knowledge of facts and so to make him " a trained observer." The greater his antecedent knowledge, the better he knows *what* to observe. A training in interpretative science tends especially to endow him with skill in reasoning. The greater his antecedent practice, the better he knows *how* to reason. Compare botany and mathematics. In botany the student's labour is expended mainly in acquiring facts ; in mathematics in acquiring skill in difficult feats of thinking. In the latter few facts are learned. Just as knowledge of the facts of one descriptive science does not imply knowledge of the facts of another, so skill in reasoning about the facts of any interpretative science does not necessarily imply skill in reasoning about facts of any other. The dexterity which comes from familiarity with the facts may be absent, or the conditions under which the thinking is conducted (*e.g.*, the degree of complication of the data thought about) may be very different. In mathematics, for instance, the data are reduced to the utmost possible simplicity ; in biology they are always very complex. Therefore, mathematicians, strayed into biology, often show a noticeable tendency to get rid of the complications by ignoring them —*e.g.*, by ignoring the immensely important Mendelian facts. As J. S. Mill noted long ago :—

" These are but samples of the errors frequently committed by men who, having made themselves familiar with the difficult formulæ which algebra affords for the estimation of chances under suppositions of a complex character, like better to employ these formulæ in computing what are the probabilities to a person half informed about a case, than to look out for means of being better informed."*

The truth of a descriptive statement is proved by the facts on which it is founded. Thus if we wish to prove the statement that man is a mammalian vertebrate, we have only to show the breasts and the backbone and our task is concluded. When the truth of an interpretative statement is in doubt, it can never be proved by

* *Logic*, III., xviii., p. 4.

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the facts on which it is founded. To quote from myself in the last number of *BEDROCK* :—

“ If we think of any object or event in Nature the antecedent of which is not already known to us, and try to account for it, two or a dozen, or it may be a hundred, possible interpretations are almost sure to suggest themselves. Any one of them *may* be true ; only one of them *can* be true ; and it is impossible to know which is true until we have observed a great deal more than the facts from which we started. In explaining, then, it is not enough to found statements on observed facts ; it is necessary also to test these statements by more and different facts and to go on testing by more and more facts till every explanation but the right one is rendered inconceivable as true.” *

Sciences vary greatly in the number of facts out of which they are built. Thus geometry is founded on far fewer facts (its axioms) than systematic botany. For obvious reasons sciences tend to become interpretative with a quickness that is in inverse proportion to the number of facts that have to be described. Thus mathematics was interpretative from its beginnings ; it consists of little else than interpretation. Astronomy did not become truly interpretative till about four centuries ago. Chemistry followed somewhat later. Biology is only now reaching the interpretative stage. By interpretation I mean, of course, not mere guessing, but proving.

Students of mathematics, physics, astronomy, and chemistry are necessarily trained in the method of interpretation. No one is able to reach distinction in these sciences who is unable to appreciate close reasoning and is not himself a close reasoner. Conclusive proof is sought with anxious care, and when offered is readily recognised as such by other workers. Consider, for example, Lord Rayleigh's recent proof of the existence of Argon. But the facts of biology are so numerous that the students of that science, the majority of whom are zoologists and botanists, are normally trained only in description. No man can acquire all the facts, and distinction and its rewards are commonly, and very rightly, reached by men who have acquired an unusual multitude of them, or who have added to the general store by discovering and describing more. The biologist, therefore, is not necessarily a trained reasoner. If anyone disputes this statement, I have only to ask, “ In what department of biology does the biologist get his training in reasoning—his training in interpretation,

* *BEDROCK*, Vol. II., No. 1, pp. 128—9.

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in linking consequents to antecedents and in proving that the links are true ? ”

Antecedent training confers on mathematicians, physicists, chemists, and astronomers identical criteria for evidence and proof. According to these precepts and practice, every verifiable fact may be used as evidence and is equal before science. Every hypothesis must be founded on and tested by facts. Proof consists in demonstrating, by means of the orthodox method of making a deductive inference of consequences, that the hypothesis under consideration accords with all the relevant facts and that no other rival hypothesis does. If the records of, for instance, the Royal Society or the British Association be examined it will be seen how carefully the students of the non-biological sciences have availed themselves of the whole of the relevant evidence and how scrupulously they have tried to show that there is no conceivable alternative to each supposition. So constantly do they offer and accept proof, so identical are their criteria, that prolonged controversies are rare, and unending controversies unknown among them. Their sciences are built up of truths that are universally accepted.

In interpretative biology, on the other hand, the criteria held by different workers as to what constitutes evidence and what proof are extraordinarily unlike. Thus, some biologists, as we have seen, regard a limitation of the evidence to that derived from experiment as the acme of scientific procedure. Others set the same value on statistics. If the records of the learned societies be examined it will be seen that in hardly a single case has an attempt been made to test hypotheses. Practically always hypotheses have been left by their authors in the stage of mere supposition. The students of descriptive science have dealt with interpretations as if they were descriptions. No more has been done than to cite the credence on which the hypotheses were founded. Very seldom has there been an attempt to *prove* by appealing to such fresh facts as furnish crucial instances. Almost the only great original thinker among biologists who has consistently applied the Newtonian method of proving is Darwin. In his works he is perpetually saying, in effect, “ If my hypothesis of Natural Selection be true, then certain other things (necessary consequences) must also be true. Now let us see if they are true.” As Newton founded his

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hypothesis of universal gravitation on facts collected by Kepler and used his own arrays of facts for purposes of verification, so Darwin founded Natural Selection on data collected by Malthus and used his own enormous collections exclusively for verification. It is very significant of biological method that not a biological interpretation can be named which is universally accepted by biologists. Among them not an important controversy has ever ended. Witness, for instance, the interminable disputes between the Lamarckians and the Darwinians, and between the Selectionists and Mutationists. Disputes cannot end unless proof is offered, and, when offered, accepted. Witness the unending disputes of religious sectarians. Every biologist, however distinguished and whatever his views, is sure to know of many men equally distinguished who disagree with him on every point of interpretation and who cannot be persuaded to abandon their opinions. I think, therefore, every biologist will agree that the reason why there is yet no universally accepted science of interpretative biology is because biologists have not as yet the common criteria which training bestows. As a consequence each man sets up private standards. Very often he believes not what he must because there is no alternative, but what he chooses; and as a rule his choice is governed by the opinions of the man who happened to be his teacher, or by those of the men who adopt the same method of discovering facts that he employs. When men do not prove their suppositions, or accept proof when that is offered, sectarianism is sure to develop.

Hence, I take it, the rhetoric, the posing as priests of "real" or "exact" or "modern" science, the depreciation of rival doctrines on irrelevant grounds, the restriction of evidence, not to verified facts, but to data supplied by some particular mode of observing, the confusing of guessing with proving, the dignified silence when proof or disproof is offered, the partisanship, the schools, the sects, the unending controversies, the total lack of universally accepted science. Hence also the incursions of the "journalists, sciologists, demagogues, and popular lecturers at large," which Mr. Elliot deplures. If such people dared to venture within the boundaries of mathematics, physics, astronomy, or chemistry, their trained students, unanimous and therefore strong, would, as I have said, take them between finger and thumb and eat them like shrimps.

THE MILK PROBLEM :

Condensation and Preservation

By Eric Pritchard, M.D.

THE economical advantages to be derived from storing and preserving so cheap and valuable a food as milk has constantly stimulated both men of science and men of business to devise practical means of achieving these desirable ends. Almost every method which has proved in any degree feasible in the laboratory has been exploited commercially, but with so little success that out of an average of 158,454,464 cwts. of milk annually consumed in the United Kingdom during the period 1899—1903 only the equivalent of 3,270,271 cwts. in the preserved form were locally produced or imported from abroad. During this quinquennial period practically the whole amount of preserved milk consisted of the condensed variety, for at that time dried or desiccated milk was absolutely unknown in this country. At the present time, although I have not the figures to prove the statement, I believe the ratio between fresh and preserved milk remains substantially the same, although there are indications that the production of condensed milk has already reached high-water mark, and that dried milk is rapidly taking its place.

For the temporary preservation of milk, that is to say, for the purpose of keeping it fresh between the time of milking and the time of its delivery at the house of the consumer, innumerable methods have been devised ; some of these depend on the application of heat to the raw product, others depend on the application of cold or on the addition of chemical preservatives ; all of them, however, owe such efficacy as they may possess to their power of destroying, or inhibiting the growth of those living organisms which are

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responsible for the fermentative or putrefactive changes which occur in milk when it turns sour, or becomes decomposed.

Inasmuch as it is extremely difficult, or almost impossible, to sterilise milk completely by any of these means, and inasmuch as milk is a most favourable culture medium for the bacteria which survive, there is no difficulty in understanding why all these methods, except preservation by cold and pasteurisation, have fallen into disrepute. The addition of chemical substances, although the latter can successfully inhibit the development of bacteria, may have consequences more serious than the dangers they are designed to prevent, and for this reason the Local Government Board has recently prohibited *in toto* the addition of all such substances to milk, and has carefully defined the amount that may be added to cream. If the proposed Milk Bill eventually becomes an Act of Parliament, even the pasteurisation of milk will become illegal, unless the statement that the milk has been so treated is clearly inscribed on the receptacle in which the milk is sold.

None of these methods for the temporary preservation of milk, not even the somewhat attractive and perfectly innocuous methods of treatment by electricity, or peroxide of hydrogen, will be considered in this article because, though of scientific and academic interest, they possess no claim to be considered of commercial or practical importance.

The sterilisation of milk by means of heat was sufficiently considered by me in the January number of *BEDROCK*, and so it only remains to discuss the respective advantages of permanently preserving milk by the methods of condensation and desiccation.

Although so early as the eighteenth century unsuccessful attempts had been made to condense milk by evaporation in the air, the first really practical method was devised by a certain Mr. Gail Borden, of White Plains, New York, who acting on a suggestion made by Professor Horsford of Boston successfully evaporated milk at a low temperature under reduced pressure ; Borden obtained a patent for his process in the year 1856.

During the American Civil War large quantities of milk condensed by this process and packed in hermetically sealed tins were supplied to the Northern armies for consumption in the field. The commercial possibilities of this new method appealed so strongly to one of

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the war correspondents accompanying the expedition that at the termination of the war he settled in Switzerland and started a small factory at Cham. This is the history of the commencement of that great industry which has since developed into the Anglo-Swiss Condensed Milk Company, and the name of the founder of the business was Mr. Charles Page, originally correspondent to the *New York Tribune*.

Although in the early days of the development of this industry almost the whole quantity of condensed milk manufactured was sweetened, nevertheless a small proportion was put upon the market without the addition of supplementary sugar. Although it might well be supposed that unsweetened condensed milk would appeal more to the taste of the majority, owing to its closer affinity with milk in its natural condition, these anticipations have not been fulfilled, and unsweetened condensed milk has never had a large sale. Until quite recently the manufacturers and vendors of condensed milk were quite exempt from the restrictions which hampered the sale of milk in its natural condition, for while condensed milk could be impoverished and adulterated to any extent, fresh milk was compelled to conform to certain arbitrary standards of purity. It was therefore quite in accordance with expectation that manufacturers of condensed milk would not be slow to avail themselves of the temptation which the invention of the mechanical separator afforded, and a very large quantity of condensed milk was put upon the market from which varying proportions of cream had been abstracted. This abuse made it necessary to include, in the year 1899, in "The Sale of Food and Drugs Act," a provision which required the specific labelling of all condensed milk prepared from skimmed or separated milk. It is commonly believed that condensed milk is practically germ-free owing to the prolonged heat to which it must necessarily be exposed in the process of manufacture. This is far from the truth, as has been proved by the researches of Drs. Gordon and Elmslie*. These observers were able to demonstrate the presence of a considerable number of living organisms in commercial samples of condensed milk, although the flora discovered did not include organisms of a pathological nature, and

* Dr. F. S. H. Coutts' Report to the Local Government Board on an inquiry as to Condensed Milks. New Series, No. 56, 1911, p. 56.

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further, in a special series of investigations, Professor Delépine,* of Manchester, has proved conclusively that tubercle bacilli, when added deliberately to milk before condensation, are invariably killed in the process of manufacture. Therefore, from the epidemiological point of view, it is clear that there are advantages in condensed milk which do not necessarily attach to the fresh product. On the other hand there are substantial grounds for believing that infants fed on condensed milk, more especially on the sweetened varieties, show less resistance to disease than do those who are fed on milk straight from the cow. And further, it must be remembered, that although thick and viscous condensed milk is an exceedingly unfavourable culture medium for bacteria, the same milk when diluted with water becomes even more quickly deteriorated than fresh milk exposed to the same conditions of contamination.

Although condensed milk is popularly supposed to possess certain economic advantages over fresh milk, this belief is entirely unfounded, for the purchaser has to pay 5½d. for condensed milk, whilst he can procure a corresponding amount of fresh full cream cows' milk and sugar for the expenditure of precisely one halfpenny less. That is to say, condensed milk is 10 per cent. dearer. Owing, therefore, to its intrinsic defects as a food, its economic disqualifications, and its characteristic taste, it is extremely unlikely that condensed milk, and especially the sweetened variety, will ever obtain a greater hold on popular estimation than it does at the present time.

At the end of the last century experiments were actively pursued to condense milk to a higher degree of concentration than had been heretofore attained; in fact, attempts were made to dry it completely. In the large butter manufacturing centres, great difficulty was found in applying the residuals to economic use, especially in those districts where there was no demand for separated milk for cattle-feeding purposes. It was this waste of the bye-products of butter manufacture more than anything else that led, in 1903, to the first commercial application of the laboratory methods of milk desiccation, which had been on trial on a small scale for some four or five years. In this year, Mr. S. Amundsen started a small factory in Kristinnia,

* Dr. F. S. H. Coutts' Report to the Local Government Board on an inquiry as to Condensed Milks. New Series, No. 56, 1911, p. 2.

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in Sweden, for the drying of milk by a process which had been invented by Dr. Ekenberg. Results, however, showed the method to be unreliable, and at the time, only capable of drying milk from which the fat had been extracted.

Shortly afterwards the Ekenberg process was improved, and other methods were devised which enabled manufacturers to produce dried milk on a commercial scale without preliminary removal of the cream. At the present time dried milk is produced in France, England, America, New Zealand, Norway and other countries in large quantities, and although chiefly used for industrial purposes, it is already winning for itself a place among certain households as a substitute for the dairyman's fresh product.

In the manufacture of dried milk there are three successful processes in actual use: firstly, the Ekenberg process, to which I have already referred; secondly, the Just-Hatmaker process; and, thirdly, that known as the Bévenot-de-Neveu. All these methods are now complicated by improvements and modifications which are protected by subsidiary patents, and indeed to an extent which is quite bewildering. Moreover, considerable secrecy is observed by some of the manufacturers, particularly with respect to the details of the preliminary evaporation of the milk before it is finally submitted to desiccation. In certain cases supplementary chemical substances, such as phosphate of sodium, saccharated lime, or glucose, are combined with the milk to facilitate the operations of drying and to render the desiccated product more readily soluble.

In the Ekenberg process the milk is sprayed under constant and even pressure on to the inner surface of a steam-heated cylinder which is kept in constant rotation. The milk is dried in partial vacuum at a comparatively low temperature, and emerges from the machine in the form of a thin ribbon or sheet. Before reaching the drying cylinder, which is maintained at a temperature which is approximately that of blood-heat, it quickly solidifies into a crystalline mass, which is then broken up and finely pulverised.

The second, or Just-Hatmaker process, which is the one most frequently employed, sprays the previously concentrated milk on to the exterior and highly polished surface of revolving steel drums. Here it is almost instantaneously dried at a temperature of 230° F., a temperature which is maintained by the circulation through the

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cylinders of compressed and heated air. This is the process which has now been made familiar to a large section of the public by frequent demonstrations at Earl's Court and other exhibitions. When dry, the milk is scraped off the cylinders by sharp knife blades, and subsequently ground up or milled into a fine, flaky powder.

The third method, which is generally known as the Bévenot-de-Neveu, but which is now so inextricably involved with the Stauff and Merrell Gere patents that it is almost impossible to say by what name it should be properly called, produces a milk powder which is altogether different and distinct from those produced by the methods already described.

By this process the milk is first concentrated *in vacuo*, and then forced under great pressure (250 atmospheres) through minute perforations in metal discs, into a capacious drying chamber, where the nebula of homogenised milk is swept across the chamber surrounded by a current of hot air by which it is instantly dried. The moisture thus evaporated is carried off as a cloud of steam, while the desiccated milk falls like fine snow to the bottom of the collecting chamber, whence it is rapidly swept up and removed.

The distinguishing feature of this method lies in the rapidity with which the milk is desiccated ; this is due to the extremely fine state of division in which the milk particles are presented to the stream of hot air. So finely atomised is the milk that even at a comparatively low temperature the moisture is almost instantaneously evaporated. Indeed, so instantaneous is the drying, that many of the most delicate and perishable liquids have been evaporated to dryness by this method without loss of flavour or colour, and have again been reconstituted by the addition of water so that the resultants could hardly be distinguished from the original liquid. In this way fruit juices, vegetable juices, meat juices, glandular extracts, and animal ferments have been desiccated, at comparatively high temperatures, without detriment to their physiological properties or to other intrinsic qualities of colour or taste. It has been claimed that the evaporation of water at the surface of the fine particles of the nebula is so rapid that a fall and not a rise in temperature must be occasioned in the solid molecules which drop to the bottom of the collecting chambers. Whether this claim is capable of proof or not, it is quite certain that both milk and whey

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can be dried by this process without detriment to the enzymes and other vital principles which exist in fresh milk.

The milk powders obtained by these several methods present quite distinctive physical and chemical qualities, which are partly dependent on the temperature to which they have been exposed in process of desiccation, and partly on other conditions of manufacture. The appearance of milk which has been dried on the surface of hot cylinders by the Ekenberg or Just-Hatmaker process, and subsequently scraped off and pulverised, is that of irregular polygonal plaques of varying dimensions and of striated structure; whereas the powder desiccated by the Bévenot-de-Neveu is very much finer and consists of rounded scales.

As for solubility, the cylinder-dried milk is easily dissolved in hot water, less easily in cold, whereas that dried by the Bévenot-de-Neveu process is equally soluble in hot and cold water. To dissolve the powder in water, an egg whisk, fork, or some such instrument should be used.

The appearances of the two kinds of reconstituted milk are somewhat different. Just-Hatmaker milk can, as a rule, be distinguished by the naked eye, partly for the reason that comparatively large fat droplets float in the surface, and partly for certain other physical differences which are difficult to define, but which probably depend on some altered colloidal condition of the solution: on standing the solution separates into quite distinct layers. Bévenot-de-Neveu milk is difficult to distinguish from ordinary milk, but when allowed to stand sometimes shows a layer of cream on the surface, like dairy milk.

As for flavour, I am bound to admit that the taste of most reconstituted dried milks is different from that of fresh cow's milk; as to whether it is more agreeable or the reverse, is a matter of individual opinion.

A few weeks ago I submitted a number of dried milks to the arbitrament of a class of lady students, asking them to adjudicate on the respective merits of the different samples. Most of these ladies were quite emphatic in their disapproval, declaring that they would sooner drink no milk at all than milks with those particular flavours. A few days later I obtained a fresh sample of milk prepared by the Bévenot-de-Neveu process and desiccated at a low temperature

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(160° F.). In the case of this milk the ladies expressed quite a different opinion ; one or two of them said they would have known that it was boiled, which, as a matter of fact, was not the case, but all agreed that they would be quite satisfied if they never drank milk of worse quality or taste.

A friend of mine who is quite an enthusiast on the subject of dried milk, but whose family does not share his optimistic views, submitted a sample of this same milk to a somewhat severer test, for he surreptitiously substituted it for the ordinary cow's milk which had been provided for the family tea. Neither my friend's wife, nor a pet cat which had also previously evinced a strong dislike to desiccated milk, detected the fraud, and it was only with difficulty that the former could be convinced of her mistake.

Unfortunately for the reputation of this particular sample of dried milk, I repeated my former experiment a few days later and asked two of my friends, who rather fancied themselves as judges of milk, to taste the reconstituted dried milk, comparing it with controls of fresh milk.

Although as a matter of fact their judgment was in error in two out of twelve separate tastings, there could be no doubt that they were confidently correct in the larger proportion of trials. It appears, then, that dried milk can be prepared by the Bévenot-de-Neveu process which when reconstituted with water cannot be distinguished in taste, appearance or other qualities from ordinary fresh milk, but that after keeping for a few days it may develop a slight taste which reveals itself to a delicate palate. It has been suggested that this taste is due to the oxidation of the fat, which owing to the fine state of division in which the cream exists in Bévenot-de-Neveu milk, offers a large surface to the action of the oxygen. Whether this is the correct interpretation or not I do not know, but personally, although I can recognise the distinctive difference between fresh milk in its native condition and reconstituted dried milk which has been kept, I should not feel inclined to impute the taste that I admit to any change in the condition of the fat. I can detect no tallowy, rancid or other fatty flavour of any kind. While freely admitting that the acquisition of this flavour on keeping is a distinct objection to the general utilisation of dried milk for table purposes at the present moment, I feel convinced we are on the

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threshold of discoveries which will enable the best varieties of dried milk to be kept without impairment of flavour for many months at a time. It has been suggested that a layer of plaster of Paris contained in a false bottom of each tin, though separated from the milk by a perforated metal partition with a suitable covering of linen or paper, might keep the milk so anhydrous that oxidation processes might be inhibited, while the substitution of nitrogen gas for ordinary atmospheric air in the tin itself has already been tried with satisfactory results.

The cost of dried milk is practically the same as that of fresh milk—but for household use it offers many opportunities for economy, since half-cream or separated dried milk can be used for cooking, and these varieties are very much cheaper. The chief advantages, however, that dried milk offers are that, as compared with so-called fresh milk, it is cleaner and freer from impurities, and that it can always be kept ready at hand for immediate use. Dried milk is in fact, as Professor Porcher says, “*La vache dans le placard.*”

In conclusion I must refer to one use of dried milk to which repeated reference is made by Professor C. Porcher in his book, *Le lait desséché*,* a book which all should read who are interested in Milk Problems. I refer to the use of desiccated milk in the feeding of infants. One quotation will be sufficient to prove that from the nutritional point of view dried milk is in no way inferior to dairy milk. At a certain Infant Clinic in Gand, in Belgium, the infant mortality rate was 260 per 1,000 in the year 1901. In the year 1903 sterilised milk was supplied to the infants and the rate fell to 150 per 1,000. In the year 1907 a house to house visitation by trained health workers was organised, and the death rate fell to 60 per 1,000. In the year 1908 dried milk was substituted for sterilised dairy milk and the rate fell to 34 per 1,000.

There is no better test of the nutritive properties of a food than to employ it exclusively on a large scale in infant feeding. Submitted to this criterion, dried milk compares very favourably with all other varieties, whether fresh or preserved.

* *Le Lait desséché*, by Professor Ch. Porcher. Published by Asselin et Houzeau Place de l'Ecole de Médecine, Paris.

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On the score of economy, of cleanliness, and convenience, dried milk possesses such great advantages over milk in the fresh state that it is almost impossible to imagine that the very slight difference in flavour which distinguishes it from the ordinary milk to which we are accustomed can permanently debar it from household use.

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[We regret that in the review of "An Introduction to the Study of the Protozoa," which appeared in the January issue, two printer's errors occurred. The name of the author should have been E. A. Minchin, M.A., F.R.S., and that of the publisher Edward Arnold.]

THEORIES OF SOLUTIONS. SVANTE ARRHENIUS, Director of the Nobel Institute of the Royal Swedish Academy of Sciences, Stockholm. (Yale University Press, New Haven; London, Henry Frowde, Oxford University Press.) 1912. xx+247 pp.

The present volume constitutes the eighth of the series of Silliman memorial lectures delivered annually at Yale University, "to illustrate the presence and providence, the wisdom and goodness of God, as manifested in the natural and moral world." The authorities of Yale University have been particularly happy in their choice of men and subjects. The Silliman lectures of Professors Sir J. J. Thomson, Sherrington, Rutherford, Nernst, and Bateson form already a series of most remarkable monographs, dealing with some of the greatest questions of science, such as the relations of electricity and matter, the integrative action of the nervous system, radioactivity, the applications of thermodynamics to chemistry, and the problems of genetics. They tell of knowledge won at firsthand by master minds, and well and truly does the harmonious beauty of this revelation uphold the wishes of the founders.

Professor Arrhenius' lectures form a fitting contribution to this great series. The man of genius who gave to the world the theory of electrolytic dissociation and ionic equilibria, and in so doing gave to chemistry as an exact science perhaps the greatest impetus it has ever received, could not do otherwise. His subject was a well chosen one. We live in a world of solutions. Everything in some degree dissolves in and mixes with everything else. The atmosphere we breathe is a molecular mixture, the rocks of the earth's crust have crystallised from solutions and are themselves in great part crystalline solutions, whilst the rivers and the lakes and the ocean itself form one vast series of ever changing solutions. Greatest wonder of all, the pulse of life throbs through a network of solutions, from the humble blade

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of grass beneath his feet to the arrogant brain of *homo sapiens*. The study of the nature and properties of solutions is therefore one of the greatest and most important problems of science. The first task of chemistry was the separation of solutions into pure substances, just as the concept of a pure substance as distinct from a solution was one of the first advances in chemical theory. And so the processes of crystallisation and distillation will always form the groundwork of practical chemistry.

But after the pure substances had been isolated and studied, it became necessary to return to the study of solutions, if only for the reason that the vast majority of chemical actions take place therein—*corpora non agunt nisi soluta*. A very large part of what is called physical chemistry has been concerned with this study, and the results have led to an enormous increase in our knowledge of and insight into the nature and mechanism of chemical actions. When one recollects that solutions of acids, bases, and salts in water conduct electricity, and that our present knowledge of such “conducting” solutions is very largely due to the inspiration and genius of Arrhenius, the fundamental importance of his work will be evident to all.

In the present work Arrhenius discusses in ten lectures our modern knowledge of the theory of solutions. They deal with the history of the subject, the modern molecular theory, suspensions, adsorption, the analogy between the gaseous and the dissolved state of matter, the development of the theory of electrolytic dissociation, velocity of reactions, the conductivity of solutions of strong electrolytes, electrolytic equilibria in solutions, the abnormality of strong electrolytes, and the thermodynamics of ionisation and solution.

Arrhenius' book is characterised throughout by the striking originality of his mind, as well as by his profound knowledge and grasp of the subject. There is a freshness and vigour about everything he says which is most delightful. Old and familiar topics appear in a new and interesting light.

It may be said without undue exaggeration that Arrhenius' monograph is of capital importance for nearly every branch of science. In the case of physics and chemistry the truth of this statement is of course obvious. But the importance is none the less in the case of mineralogy, geology, physiology, medicine, and engineering. All these branches of science have to deal with solutions. The genesis of a mineral, the weathering of a rock, the excitation of a nerve, the deposition of a calculus, the corrosion of a metal or a building stone, the formation of a boiler scale—what can be said of these by him who is ignorant of the theory of equilibria (and particularly electrolytic equilibria) in solutions?

Yet few medical men and very few engineers have any real knowledge of solutions.

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In an extraordinary degree this science of solutions has exercised a beneficent influence on almost every branch of "natural philosophy." The authorities of the Silliman foundation could have chosen no better exponent than Arrhenius, and his "Lectures on the Theories of Solutions" are destined to bear rich fruit in many fields.

F. G. D.

INSTINCT AND EXPERIENCE, by C. LLOYD MORGAN, F.R.S. (London: Methuen & Co., 1912.)

A work by Professor Lloyd Morgan on such a subject as instinct and experience is certain to be interesting and valuable; and this reasonable anticipation is justified in the present book. Indeed, being for the moment well out of range of any missiles in my readers' neighbourhood, I may venture to affirm that the instinct which led me to the book is borne out by the experience of it which I now possess. But there is much in it which invites criticism, and in the short space here available that criticism must be of the most general character.

The book is written to propound a theory. Now, when a writer propounds a theory on a highly complex subject, it becomes interesting to know the general tendencies of his philosophy and how his mind is orientated with regard to current problems. On this, as on all other psychological topics, an incredibly vast amount of nonsense has been written. The matter is alleged to be outside the sphere of science: the metaphysicians say it is their province; and there are other competitors too. Even the *Spectator* has pegged out a claim to be heard on the subject. It is clear, therefore, that a writer on this much-abused problem must be extensively eclectic in dealing with contemporary theories. The vast mass of what is written being rubbish, a consideration of it is not only a waste of time, but a positive obstacle to progress. According to an author's philosophic proclivities, he will pick out the theories and the writers about whom his own account is centred. And a sound philosophy is thus of immense value, for it gives its possessor the power to grasp and co-ordinate the essentials of the subject, while ignoring the rest.

Judged by this standard, Professor Lloyd Morgan's book falls short lamentably. The writer to whom he pays chief attention is M. Henri Bergson. Now M. Bergson's ideas of instinct are so purely metaphysical that it may be safely affirmed that whatever theory may hereafter be adopted by science, it will be one which has not the slightest reference to anything written in M. Bergson's works. Further, for a complete and exhaustive study of instinct, the scientific student has no real occasion whatever even to glance at any of M. Bergson's writings. His so-called philosophy is a fashion, which, like other fashions, soon passes, but which while it lasts presses imperiously for recognition.

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Professor Lloyd Morgan, while often disagreeing with him, takes M. Bergson pretty much at his own valuation, or that of his lady disciples ; and in centring his book about that philosopher's doctrines, he has got the centre of gravity of his theory seriously displaced. Space is also given to Dr. C. S. Myers, Dr. McDougall, and Dr. Hans Driesch. The first two naturally demand discussion, but Dr. Driesch might well be omitted. Darwin, Huxley, Sir Ray Lankester and Professor Sherrington are each referred to once only in the course of the book ; Spencer and Bain not even that. Yet all of these have dealt with the subject of instinct, and it is difficult to understand how a book can have been written which pays them so little attention. It is evident both by its omissions and its commissions that Professor Lloyd Morgan's theory has somewhat shady relationships. And unfortunately it is probable that many men of science will, in consequence, attach less importance to the book than it deserves ; they will be repelled moreover by its metaphysical terminology, such as the definition of instinct as " organic behaviour suffused with awareness," which is calling it names rather than defining it. They will not understand the statement that " the fully explicit logic of human reason is but a higher development of the scarcely explicit logic of perceptual intelligence ; and this in turn has its roots deeply embedded in the implicit logic of instinct." The Bergsoniennes may rejoice, but the man of science will conclude that this is no concern of his ! Finally, Professor Lloyd Morgan is very weak on the subject of vitalism. Now, for any psychological discussion, this is a matter of the greatest importance. Mechanism lies at the basis of psychology, as the conservation of energy does at the basis of physics : it is a principle of the most enormous heuristic importance ; and I venture to express the emphatic opinion that no one who does not think in mechanistic terms is likely to get very far with any sort of psychological problem.

While, therefore, Professor Lloyd Morgan's philosophic orientation is decidedly unfortunate, there is much of real importance in the detail of the book. It is for his extensive study of details that readers will have recourse to this book. Were it not for the unfortunate mode of presentation, the book might almost have been looked upon as indispensable.

HUGH S. ELLIOT.

SEX ANTAGONISM, by WALTER HEAPE, F.R.S. (London : Constable, 1918.)

The main purpose of this book is to attack the two central theories of Dr. Frazer's *Totemism and Exogamy*, and to suggest other theories as to the origin of those institutions. The method by which Mr. Heape attacks the problem is radically different from that of Dr. Frazer. Dr. Frazer treats the subject purely as an anthropologist, and follows the

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approved methods of inductive science. That is to say, he starts with a vast accumulation of observed facts—so vast indeed as to be a constant source of wonder to his readers ; and upon these facts he proceeds to found such generalisations as seem to him warranted. Mr. Heape pursues precisely the opposite method. He is not an anthropologist at all. He is, on the other hand, an acknowledged authority on sexual physiology ; he starts from certain general principles established by that science, and proceeds to deduce from them the instincts which in his opinion inevitably give rise to the two institutions of Totemism and Exogamy. And, unfortunately, the conclusions of inductive anthropology turn out to be very different from those of deductive physiology. It is true that the deductive method is looked upon with grave suspicion in all the sciences subsidiary to biology ; but in the present case, the methods of the two authors differ more in appearance than in reality. In each case a theory is formed as a pure hypothesis, and tested by comparison with the facts. Mr. Heape's theory is one furnished him by his own science ; Dr. Frazer's was invented by him for the occasion.

Descending from generals to particulars, we can scarcely do more than express individual preferences as to the rival theories. Exogamy is the institution which compels men to seek wives outside the clan or tribe to which they themselves belong. Dr. Frazer explains it as a piece of intelligent legislation on the part of primitive men, in order to prevent the possibility of incestuous mating. This theory appears, on the face of it, to be much more closely allied to the discredited old ideas of a social contract, than to modern ideas of evolution. However this may be, Mr. Heape attacks Dr. Frazer with much success ; and here he is likely to find widespread agreement among anthropologists. His own theory is based upon the principle that men are of roving inclination, and prefer to mate with women removed from their own clan or nation. This instinct to seek abroad for a mate lies, in Mr. Heape's opinion, at the basis of the practice of exogamy. Mr. Heape's suggestion is of great interest and importance. The weakest point in it seems to be that, while furnishing a clue as to the physiological grounds of exogamy, it does not explain the definite prohibition of endogamous unions, which I take to be one of the most essential features of exogamy. Mr. Heape's theory might well be held in conjunction with that of Dr. Westermarck, which has hitherto appeared to me to hold the field. Dr. Westermarck accounts for the horror of incest, and for the more moderated dislike of endogamy, by means of Natural Selection. Those tribes which possessed the exogamous instinct would survive and flourish, while those which had no such instinct would weaken through incestuous unions and finally disappear. Mr. Heape, while accounting for exogamy by reference to the demand of the male for variety and novelty, accounts for totemism

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by the desire of the female to restrain the errant instincts of the male. He holds, therefore, that exogamy is an institution springing from male instincts, and totemism from contrary female instincts. Mr. Heape next devotes a chapter to the somewhat hopeless task of trying to find evidence in favour of maternal impressions; that is to say, the theory that mental impressions made upon a pregnant woman will result in corresponding physical marks upon her offspring. Mr. Heape tells us that the belief in maternal impressions is common among breeders. But so also is the closely-allied belief in telegony, which has now been definitely refuted by Professor Cossar Ewart's Penicuik experiments. I fear that even Mr. Heape's authority will not suffice to revive a theory which has now been generally relegated to the sphere of superstition.

As to modern sex antagonism, Mr. Heape is extremely interesting and suggestive. The modern woman's movement is a product of unsatisfied sexuality, enforced by social conventions on a large proportion of the female population. The class of spinsters is, in consequence of these conventions, a more or less pathological class. They are, moreover, biologically superfluous; parasitic upon the race, to which they cannot contribute. Female suffrage would, therefore, in Mr. Heape's view, involve the enfranchisement of a large pathological section of the community, living under unnatural and unwholesome conditions. That the woman movement springs from suppressed or perverted sexuality is, I suppose, scarcely any more a matter of doubt. No other physiological source would be strong enough to inspire so passionate an intensity in their propaganda. Their thoughts, talk and writings, both private and public, are of sexual matters. Their acts are the product of an overwhelming instinct; and, as in all such cases, are illogical and comparatively unpurposive. I refer partly to their methods which are ill-adapted to achieve their end; partly to the end itself; for no rational estimate of the value of the vote would suggest that it can make any considerable difference in the lives of women.

While, therefore, I regard the woman's movement as one of the most significant facts of our age, there seems little reason to suppose that it will continue in its present form for very long. The duration of a physiological storm varies inversely as its intensity. Other epidemic-hysterical movements, such as tarantism, flagellism, etc., having the same external appearance and the same physiological foundation as our woman's movement, have been common enough in the past, and have generally worked themselves out within a few years. In the meantime there seems little need for the alarm felt by nervous people. The attacks on property are raised to a fictitious importance by the wide publicity they receive. These offences make practically no impression whatever on criminal statistics. They are infinitesimal additions to the annual output of crime in the country.

Mr. Heape deals with sex antagonism from a scientific and rationalist

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aspect. In this respect the book embodies a point of view which makes it one of the most important works on the subject that have yet been published in England.

HUGH S. ELLIOT.

A MONTESSORI MOTHER. By Mrs. D. C. FISHER. (London : Constable & Co.) 800 pp., with many Illustrations. 4s. 6d. net.

The rapidity with which the ideas and methods of Madame Montessori have been spread abroad in Europe and America is a marvel, even in these days of exploitation. Partly this may be attributed to the clever management of publishers and advertisers : but the new cult would scarcely have secured such support, if as some people suppose, it offers nothing more than a series of parlour tricks for babies. No educational "reform" during our generation has excited such ardent devotion among adherents, or such bitter opposition from unbelievers. The bitterness of the conflict may be attributed in part to the founder herself : Madame Montessori is a genius and an apostle : she also possesses a really scientific mind and has unquestionably produced scientific work of the first quality : but—she cannot write a book. To expound one's own researches is not an easy task in any realm of science ; in education the difficulties are all the greater, since the teacher cannot easily escape from absorption in his own ideas, and look at cause and effect with detachment, or compare his own results with the work of others. In other words, Moses usually needs an Aaron to interpret his vision to the people ; now in the present case *A Montessori Mother* provides exactly what is required. The authoress makes no pretensions to literary craft : she writes as "an ordinary American parent, desiring above all else the best possible chance for her children." She realises the sort of questions that other parents are everywhere asking about "Montessori," and she has put together a series of chapters in which these are answered with the utmost clearness and sympathy.

Although the writer hails from the United States, there is nothing specially American in her style, but in order to assist the English edition, a long introduction has been supplied by Mr. Edmond Holmes. This is the weakest part of the book : Mr. Holmes repeats his eulogies of "Egeria," whose village school had previously enabled him to "find salvation" : he proclaims himself with great gusto as a heretic "arrayed against a formidable host of vested interests," but his attempts to range Montessori with Plato, Buddha and Shakespeare are irritating rather than enlightening.

There are three features in Mrs. Fisher's exposition which are worth close attention. Firstly, she has realised that the Montessori apparatus is not a patent medicine, but that its essential elements can be traced in

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the thousand and one devices which mothers, nurses and children of all countries have empirically adopted. Madame Montessori's merit consists in having made orderly and scientific what without her insight is scrappy and adventitious. In other words, the Montessori toys form a genuine contribution to science simply because their inventor has studied the child for years and has based her procedure upon the results of trial and failure extending over many years. The homely illustrations which Mrs. Fisher supplies of children's behaviour in nurseries, long before Montessori was heard of, do not in the least diminish the greatness of her genius, but, in the eyes of sober judgment, will enhance it.

Secondly, this same comparative method is followed in the chapter devoted to the kindergarten. Madame Montessori's own writings show little acquaintance with Fröbel, although some of the most distinguished of his disciples did pioneer work in Italy. And it is the Fröbelians, so-called, in England and America, who have been most rigorous in attacking the new doctrine; to believe in "Montessori" appears to them an affront to the kindergarten. Now Mrs. Fisher makes it clear that while the two systems have something in common, they do stand apart not only as regards the time table, *i.e.*, in the pursuits on which the child's day should be engaged, but still more in the attitude and spirit of the infant teacher. To explain the situation she goes back to Fröbel's life and then extends her survey to the conditions which have prevailed among English and American families in order to see in true perspective the defects and benefits of the kindergarten. "Not foreseeing the masterful women, with highly developed personalities, who were to be the apostles of his ideas in America, . . . it did not occur to Fröbel that there was any special danger in this direction." This chapter by itself makes the book worthy of publication. It is critical and outspoken, but entirely sympathetic. The sum of the matter is that the infant teacher tries to "teach" while Madame Montessori bids the teacher leave the child alone, when once you have put a suitable toy (or rather a selection of toys) within his reach.

This brings us to the third, and perhaps most important, feature of the book. Since each child is to be busy with his own apparatus, it is clear that the regular machinery of school and schoolma'm, with collective lessons, becomes of minor importance. The nurse, governess, mother, with a few children or even only one, can make use of Montessori. In other words, the appeal lies to the family quite as much as to the school. Of course, Fröbel understood this thoroughly, and it is singular that Madame Montessori (possibly because of some distinctive qualities of Italian home life?) seems to ignore it. But Mrs. Fisher is Anglo-Saxon: she knows that educational reform with us must seek its support not only in the scholastic profession, but in the sympathetic understanding of an intelligent laity.

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We have not attempted in this notice to review either the Montessori system or the philosophy which underlies it : that topic requires larger room for adequate treatment. Mrs. Fisher's service is to have presented the whole matter in a form both practical and comprehensive. We stated above that the authoress makes no pretensions to literary craft, but she writes with ease and grace ; both the arrangement and the style are in sharp contrast to the dull exposition of most writers on educational themes. She has produced a good book because she had something to say ; and has said it with the charm of a cultivated woman who has taken pains to make her message clear.

J. J. FINDLAY.

PSYCHOLOGY AND INDUSTRIAL EFFICIENCY, by HUGO MÜNSTERBERG.
(London : Constable & Co., Ltd. ; Boston and New York :
Houghton, Mifflin Co., 1918.) Pp. viii + 816.

If a British dog-in-the-manger patriot were reading this book, one can imagine him laying it down at the end of Chapter IX. and offering up two mutually complementary prayers : (1) that Mr. Winston Churchill should read, mark, learn and inwardly digest it ; (2) that it might escape the notice of the Kaiser. The chapter refers only to the mercantile marine it is true, but should the methods therein provisionally suggested weather the further experimental tests that inevitably await them, their application to the *personnel* of our senior fighting service would become at once a matter of imperial concern. Indeed, it may be asked with all seriousness even now whether our Admiralty ought not to establish a psychological laboratory of their own at once, so as to be able to carry these " further experimental tests " to a decisive finish with the least possible delay. The results to be arrived at *might be* so enormously important !

The particular test experiments described in the chapter referred to, originated in the following manner. Professor Münsterberg was approached by one of the largest shipping companies

" with the question whether it would not be possible to find psychological methods for the elimination of such officers as would not be able to face an unexpected suddenly occurring complication. The director of the company wrote to me that in his experience the real danger for the great ships lies in the mental dispositions of the officers. They all know exactly what is to be done in every situation, but there are too many who do not react in the appropriate way when an unexpected combination of factors suddenly confronts them, such as the quick appearance of a ship in a fog. He claimed that two different types ought to be excluded."

These types are : (1) those who are rendered incapable of prompt action through mental paralysis ; (2) those on whom the necessity for

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immediate action has so powerful an effect that they put into execution the first idea that leaps into their minds. The third and desirable type includes

“the men who in the unexpected situations quickly review the totality of the factors in their relative importance and with almost instinctive certainty immediately come to the same decision to which they would have arrived after quiet thought. The director of the company insisted that it would be of highest importance for the ship service to discriminate these three types of human beings, and to make sure that there stand on the bridge of the ship only men who do not belong to those two dangerous classes. As the problem interested me, I carried on a long series of experiments in order to construct artificial conditions under which the mental process of decision in a complicated situation, especially the rapidity, correctness and constancy of the decision, could be made measurable. . . . It seemed necessary to create a situation in which a number of quantitatively measurable factors were combined without any one of them forcing itself on consciousness as the most important. The subject to be experimented on has to decide as quickly as possible which of the factors is relatively the strongest. . . . As usual, here, too, I began with rather complicated material, and only slowly did I simplify the apparatus until it finally took an entirely inconspicuous form.”

This apparatus consisted of twenty-four cards, the size of playing cards, on the upper half of which were printed, in four rows of capitals, the letters A, E, O, and U in irregular repetition. On each card one of these four vowels appears with greater frequency than do the other three. On four cards this excess is 12; on eight it is 8; on eight it is 4; and on four it is 8, while “eight different consonants are mixed in.” These twenty-four cards have to be distributed as rapidly as possible into four piles in accordance with the predominance of each of the four letters. The time taken (measured in fifths of a second) and the number and character of the mistakes determine the result. Attempting to count is useless; it invariably leads to disaster.

“I have made the experiment with very many persons, and results show that those various mental traits which have been observed in the practical ship service come clearly to light under the conditions of this experiment. Some lose their heads entirely . . . their attention is pulled hither and thither so that they feel an inward paralysis . . . other subjects distribute the cards at a relatively high speed . . . any small group of letters which catches their eye makes on them, under the pressure of their haste, such a strong impression that all the other letters are inhibited for the moment and a wrong decision quickly made. Finally, we find a group of persons who carry out the experiment rather quickly, and at the same time with few mistakes. It is characteristic of them to pass through it with the feeling that it is an agreeable and stimulating mental activity. . . . Everyone has at some time come into

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unexpected, suddenly arising situations. . . . They know quite well that they could not come to a decision quickly enough, or that they rushed hastily to a wrong decision, or in just such instants a feeling of repose and security came over them and with sure instinct they turned in the direction which they would have chosen after mature thought. The results of the experiments in sorting the cards confirmed this observation in such frequent cases that it may be hoped that a more extended test of this method will prove its practical usefulness."

And now, with the above concrete illustration to light us on our way, let us turn our attention to the Professor's book as a whole. In some quarters its ultimate significance has lacked appreciation. It has been asserted that the only result of the increased industrial efficiency Professor Münsterberg aims at would be to give the trusts and millionaires a bigger "pull" than ever. Well, let us assume for the moment that it would be so; nay, more, let us even imagine chattel-slavery reinstated as the economic basis of Society. Would it mean nothing to those slaves to have allotted to them, through the skill of trained psychologists, the tasks they most liked doing, the labour they were most fitted to perform? From the practical point of view, however, this last reflection is entirely without relevance. For social evolution is *not* tending towards slavery, that intensest form of economic parasitism, but in precisely the opposite direction.

As in the last few centuries behind us, so in those immediately ahead will an ever-increasing diversity of industrial tasks confront humanity. For those tasks to be, *as a rule*, efficiently and cheerfully performed depends *inter alia* on the fulfilment of the three following conditions. It must be made possible to determine: (1) what mental qualities are demanded by any particular task; (2) whether such required qualities are possessed by any selected individual; (3) whether, though discovered to possess them, such selected individual would find such task an uncongenial one. As an illustration of the third condition it might be mentioned that monotony and variety of employment may, each of them, be either agreeable or disagreeable: this depending entirely "upon the special disposition of the individual" (pp. 198 *et seq.*).

To afford the world a reasoned assurance that these three conditions *are* attainable, may be said to be the chief *raison d'être* of Professor Münsterberg's book. What this assurance might mean to mankind in terms of potential well-being it would be impossible to overestimate. Think, for example, of the millions who have gone under, through failing to choose a vocation suitable to their own inherent capabilities—misfits *that might have been* fits!

No doubt it would sound perilously like "slopping over" to talk of the dawn of a new era. And yet a thing has happened, and happened

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quite recently, that has never happened before. The myriad workaday problems on whose solution the bulk of human happiness depends has come within the ken of a great psychologist; and poor stumbling humanity has been *told* of a new light wherewith to guide its hitherto uncertain footsteps. It remains now for the world's statesmen to make that light a reality.

From whom will its inventor gather his reward? Probably, unless precedents are worthless, from posterity! By that time the bibliography of psycho-economics will be something huge—and a copy of this, the pioneer volume, worth thrice its weight in gold.

THE HERMIT OF PRAGUE.

THE TERATOLOGY OF FISHES, by JAMES F. GEMMILL, M.A., M.D., D.Sc.).
(Glasgow: J. Maclesose & Sons, 1912.)

In the introduction to this handsome quarto volume we are told that its primary object is to throw light on the structural aspect of the major abnormalities occurring in fishes, particularly in the trout and salmon. Dr. Gemmill has carried out his task with admirable thoroughness. The abnormalities are defined and classified, detailed descriptions are given of the author's own observations, and these are compared with previous records gathered from the extensive literature on Vertebrate Teratology. Twenty-six fine plates and many text-figures illustrate the work. Perhaps the most striking outcome of such a study of teratology is the conclusion that the abnormalities are not so irregular or chaotic as might be imagined; most of them can be reduced to a comparatively small number of definite types. The major abnormalities fall into three groups: double monstrosities, triple monstrosities, and cyclopia. To each of these a chapter is devoted; while the last chapter deals with minor abnormalities, such as hermaphroditism, defects of the skeleton and coloration. The author concludes that each of the recognised types of abnormality can arise in a spontaneous manner by abrupt germinal variation, in which case they are capable of transmission to descendants when reproduction is possible; and, further, that most of the recognised types of monstrosity can also be produced by environmental factors acting during the course of development. The deviation from the normal in the first case is due to some instability in the constitution of the germ-cells, and in the second case to some new external stimulus; but in both the range and nature of the abnormality is determined by the limited potentialities of the organism. In fact, monstrosities are unusually large variations, and no hard and fast line can be drawn between abnormal and normal variation. The general reader will be interested in the study of inheritable monstrosities because they afford a ready proof of the

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efficacy of natural selection ; for, since they constantly occur, they must be as constantly eliminated, as they rarely or never persist to form new races. The polyembryony of the Armadillo seems to be one of the few known instances of the preservation of such a major abnormality. But we are now straying beyond the scope of Dr. Gemmill's interesting volume.

CURRENT RESEARCH NOTES

Physiological Adaptation to Low Barometric Pressures.

IN 1911 an expedition was planned by some members of Oxford and Yale Universities with the object of making a renewed study of the physiological adaptations presented by those who reside at such levels that they are subjected to low atmospheric pressures. The members of the expedition were J. S. Haldane and C. G. Douglas, of Oxford; Yandell Henderson, of Yale; and E. C. Schneider, of Colorado, and a detailed account of their observations and conclusions have just appeared in the *Philosophical Transactions* of the Royal Society. The publication of *Hohenklina und Bergwanderungen*, by Zuntz, of Berlin, and his colleagues Loewy, Müller and Caspari, in 1906, treated the entire subject of physiological adaptations at high altitudes at great length, and now, a few years later, this monograph, by Haldane and his fellow-workers, considerably adds to our knowledge, but at the same time compels a suspension of judgment as to the validity of several of their conclusions. It is not, however, proposed to do more here than indicate the most interesting of their results.

The observations were carried out on the summit of Pike's Peak, in Colorado. The height is 14,109 feet above sea-level, and the actual rooms in the summit house, which served as a laboratory, were about twenty feet lower. The Peak was free from snow.

The essential cause of mountain sickness, a condition first accurately described by the Jesuit, Acosta, in 1580, as suddenly overtaking those who attempted to climb the Andes, is lack of oxygen; other factors which separately or together have been suggested in explanation, such as diminished mechanical pressure on the organs of the body, a lowered temperature, or a deficiency of carbon-dioxide in the air, which is the theory suggested by the late Professor Mosso, of Turin, are quite negligible, and there is no need to assume any other essential cause for the appearance of such symptoms as blueness of the lips, headache, nausea and sickness, together with an altered type of respiration, than a diminished amount of oxygen in the respired air. In consequence of this, before acclimatisation occurs, there is a definite fall in the pressure of oxygen in the arterial blood, and the effects of this want of oxygen become at once apparent directly an individual

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undergoes even moderate exertion ; an exaggerated rate and depth of the respirations and blueness of the lips are at once noticed.

Signs of acclimatisation, or, in other words, an adaptation of the individual to the altered barometric pressure, which is about 460 millimetres on Pike's Peak, appear two or three days after reaching the summit. The whole process of acclimatisation proceeds rapidly during the first two or three days, but takes several weeks to become complete. It is therefore obvious that the symptoms observed in balloon ascents are far more serious than in acclimatised persons, and life is threatened at a correspondingly smaller diminution of pressure.

Three factors are considered to be concerned in the process of acclimatisation. The first is an increased activity of the cells, which line the depths of the lungs, and this stratum of cells has been calculated to possess an enormous area, as much, indeed, as 90 to 100 square yards. A second, is an altered type of respiration dependent upon a fall in the pressure of the gaseous contents of the air within the lungs, and consequently the pressure of carbon-dioxide gas is found to be about two-thirds of the normal value at the sea-level. Since it is the pressure of this gas within the lungs which regulates the frequency and depth of respirations, this fall in pressure gives rise to a corresponding increase in the actual volume of air breathed, and if this is compared with the quantity breathed at sea-level it is found to be increased at 14,000 feet. For short periods, during which considerable muscular work was performed, the volume of air respired is about three times as great as for equal exertions at the sea-level.

The third factor in acclimatisation is an absolute increase in the percentage of hæmoglobin, which is the essential colouring material of the blood, in virtue of which this liquid can normally take up from the air within the lungs that oxygen which is necessary for existence ; a process which must continue almost uninterruptedly throughout life, since the available oxygen within the body is about half a litre and suffices for only three or four minutes' existence.

It is only necessary to turn to the physiological conclusions of Dr. Filippi, which are given of the expedition of the Duke of the Abruzzi and his party in *Karakoram and Western Himalaya*, to recognise the divergence of opinion as to the cause of the phenomena exhibited by those who climb to great heights. Some members ascended to 24,600 feet. This corresponds to a barometric pressure of about 312 millimetres, or 12 inches of mercury. Since the ascent was made without exhaustion, without difficulty of respiration, and an absence of headache or nausea, after acclimatisation at about 17,000 feet, Dr. Filippi does not regard the diminished pressure of oxygen as of much moment, or, at any rate, as the essential cause of mountain sickness. This is probably, indeed almost certainly, a mistaken view, and one which is wholly unsupported by the observations carried out on Pike's Peak.

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The chief difficulty in Haldane's theory of acclimatisation is presented by a consideration of the cardinal factor which he advances as an explanation, the behaviour of the living cells which form the actual surface of the lungs, and which in a single thin layer separates the blood from the air. In normal conditions at the sea-level the relations which obtain between the blood and air are such that the gas simply diffuses through the stratum of living cells from the air into the blood, and this simple physical process amply suffices to supply the body with all the oxygen which is necessary for existence. Residence at a high level with less oxygen in the respired air, however, on the theory suggested by Haldane, brings into play a function of the lungs which is of such a nature that the previously inert or neutral stratum of cells, the lung epithelium, actively picks up oxygen and pushes this into the blood. In other words, at high altitudes this gas is actually driven into the blood by the activity of the lung epithelium. This view is the logical deduction from their experiments, for, whereas at or near the sea-level the oxygen pressure in arterial blood is no higher than that in the depths of the lungs, at a height of 14,000 feet, the oxygen pressure of the blood may be no less than 85 millimetres of mercury *above* the pressure of that gas in the air within the lungs. This conclusion that a function which is exhibited, not at rest, but during muscular activity, and one which is not noticeable in man at the sea-level, but comes into play so that man may become acclimatised to the rarefied condition of the atmosphere at high altitudes, however, is difficult to accept, and it does not appear probable that in the evolution of the functions of such an organ as the lung, a specialised mode of activity should only be exhibited in such a marked degree at diminished atmospheric pressures.

GEORGE A. BUCKMASTER.

NOTES ON NEW APPARATUS

An Ice-Cradle

WHEN the temperature of the body becomes dangerously high one has to resort to various means to lower it, such as drugs, tepid sponging, baths, etc. Against all these methods there are objections. Drugs take time to act, and may be uncertain; whilst sponging or bathing, even when carefully done, cause much discomfort to the patient.

Miss Braidwood, of Colchester, has tried to improve on previous methods by inventing an ice-cradle, now put on the market by Messrs. Down Brothers. This cradle is placed in the bed over the patient's body, and to it are suspended several small pails filled with ice and covered with removable flannel caps to prevent condensation and dropping of moisture. The temperature can be regulated according to the number of pails used and the quantity of ice.

Much can be said in favour of this apparatus; it causes no discomfort to the patient, can be managed easily by one nurse, saves time and much trouble, and, when not in use, can be folded up flat and easily packed away. It is undoubtedly a step in the right direction towards efficiency without distressing the patient; but we do not think it ideal, and can prophesy that it will be improved upon as soon as the manufacturers are assured of the demand for such an article. A system of Leiter's tubes connected with a cold water tap, and an ice jacket, laid over an ordinary cradle, would serve probably as well in relieving the patient, with a minimum of discomfort for him and greater simplicity for the nurse.

Hypodermic Medication

It has been recognised that taking a long course of medicine by the mouth is a cumbersome method of treatment. People often weary of it and discontinue it through laziness or neglect. Men in the Services, the army particularly, are in this respect their own worst enemies. Attempts have been made to replace frequent dosage by the mouth by infrequent injections under the skin. The first principle of hypodermic medication is *asepsis* both in technique and drug.

As regards technique the administrator relies on his own efforts for

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efficiency, and this can be attained by proper precautions. With regard to the drug the administrator has to rely on others, and is therefore not so confident.

To meet this difficulty certain firms have adopted a method for acquiring *asepsis* such as that used in Chamberland's filter. By passing through certain filter materials in the neck of the phial containing the drug a fluid is rendered sterile. The fluid to be injected for hypodermic medication is so treated, and the administrator is enabled to give drugs hypodermically without any fear of abscess formation as a result of the introduction of *sepsis*.

Hot Dressings by Electricity

For many ages heat has been used to enable living tissues to cope with the inflammatory changes besetting them. In recent years this purpose has been gained by means of "hot dressings." But these dressings are wet, and make the part sodden. Also it is felt that the very small amount of antiseptic used in the lotion to prepare the dressing placed on the surface could do but little good, and that the only benefit of these dressings was in their heat. There is now on the market in London an apparatus which, unlike hot dressings, will give any heat constantly. This Holmquist Electrotherm, as it is called, consists of a flexible metal coil protected by two or three layers of flannel. When connected with the electric installation, as by a lamp bracket, the metal coil, owing to its resistance to the current, gets hot. The amount of heat generated in the coil is controlled by a switch. So far the apparatus is ideal, for the coverings of the coil are easily removable, washable, and replaceable. But there are at least two warnings which show that this apparatus must be used with care. First, there is no *visible* sign that the current is "on" or "off." It may therefore be very easily left on by accident. Second, if the coverings of the coil get moist they will conduct both heat and electricity, with a result that the patient may be burned or receive shocks, the coverings may be set alight, the safety fuses vaporised, etc. Still, with common sense and care, this apparatus can be used with great benefit in many conditions.

On a principle similar to the above the benefits derived from massage may be increased by using a roller, brush, or comb connected with a battery. Electricity has been used domestically in many ways; for instance, the iron, the oven, the hat pad, and the steriliser. In every case its use is a great benefit, but it requires more care than the older methods, and each device may cause trouble, expense, or danger through the current being "left on." No indicator has been added to them to show visibly whether the current is on or off. Thus many electric sterilisers are "burned out" every year, causing a loss of time, temper and money. All these adaptations of the use of electricity

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are in the immature stage as yet, and, though convenient, must be improved before they are acceptable for the use of the public.

The Holmquist Electrotherm is free from these stigmata, as it will be used only by doctors, nurses, or those competent to be in immediate charge of the sick.

Mechanical Bedsteads

The ordinary bedstead, which has been evolved from a resting-place on the ground, may be perfect for the use of those in health, but usually is very defective for the attentions required by persons confined to bed. To meet this want, firms are always producing bedsteads, like the Surcar Mechanical Bedstead, fitted with contrivances to allow the patient to be raised or otherwise moved, and to sit up without effort on his own part, with the aid of but one nurse. These bedsteads are necessarily complicated and expensive. As they are useful only at the time of illness, nobody wishes to keep them. Naturally the firms which produce them find this part of their business not very lucrative, and necessarily put so high a price on them that a profit is made on a single transaction. Should such a bed be acquired with the object of hiring it out to those who may require it, it is found that the mechanical arrangements quickly depreciate, getting stiff, creaky, smelling of oil, and so forth. Hence the life and market for "Mechanical Bedsteads" must be very limited.

A Combination of Chair, Couch and Operation Table

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No. 3.

OCTOBER, 1913.

VOL. 2.

THE EARTH'S MAGNETISM*

By L. A. BAUER, M.A., Ph.D., D.Sc.,
Director, Department of Terrestrial Magnetism, Carnegie
Institution of Washington

It is indeed a great privilege and pleasure to give a lecture at Oxford, where Edmund Halley, whose name the Founder has so wisely coupled with this Lectureship, laboured devotedly in the interest of science; and to be permitted, in some small measure, to pay the debt of Terrestrial Magnetism, and my own personal debt as well, to this illustrious investigator.

Halley's varied scientific activity and his wide sympathies were well set forth by the Halley Lecturer † of two years ago, who had as his subject an astronomical one—"The Stars in their Courses." The theme of last year's lecture, ‡ "Large Earthquakes," by that zealous pioneer, Professor Milne, again exemplified both the scope of this Lectureship and the fact that Halley's interest and achievements in geophysical science, though not generally so well known as his astronomical discoveries, were no less great. The subject of the lecture to-night—"The Earth's Magnetism"—is one in which Halley's name stands out pre-eminent among the early students of the science. As it is a large subject and one in which there might be much discursive rambling, we shall do well to limit ourselves

* The fourth "Halley Lecture," delivered in the Schools of the University of Oxford, on May 22nd, 1913; illustrated by lantern slides.

† Professor H. H. Turner, D.Sc., D.C.L., F.R.S., Savilian Professor of Astronomy, University of Oxford (see BEDROCK, Vol. I., No. 1, April, 1912, pp. 88—107).

‡ Published in BEDROCK, Vol. I., No. 2, July, 1912, pp. 137—156.

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somewhat—to choose our starting point and then proceed in certain definite directions.

The adopted flag of the Chinese Republic consists of five stripes, partly because, as I am told, in China all good things are five—five seasons, five principal grains, five genii, five relationships that make up life, and five points of the compass: north, south, east, west, and *centre*. For, to the Chinese, the starting-out point is as important as the point to which, or direction in which, a journey is made. So it also must be with us to-night.

According to the regulations governing this lecture, it is to be known as the “Halley Lecture on Astronomy and Terrestrial Magnetism.” “Astronomy shall include Astrophysics, and Terrestrial Magnetism shall include the physics of the external and internal parts of the terrestrial globe.” This lecture might, therefore, with propriety cover the whole range of investigation in terrestrial and cosmical magnetism. However, we must limit ourselves to those particular lines of research in our subject in which Halley himself was chiefly interested. It so happens that these are the very lines also in which I have been given the opportunity to continue and expand the work begun by him.

After Halley had made two attempts to establish a working theory respecting the distribution of terrestrial magnetism and the cause of its striking change with the lapse of years—the so-called secular variation—he must have reached the conclusion that the elusive problem of the Earth’s magnetism would be more profitably advanced by additional facts than by further speculation. That, paraphrasing Seneca, to avoid making a false calculation of matters, it were better to advise with Nature rather than with opinion. Accordingly we find him setting out in October, 1698, in command of a sailing ship, the *Paramour Pink*, and cruising in her under orders from the British Government, back and forth, north and south, in the Atlantic Ocean for two years, observing almost daily, sometimes several times in a day, the angle which the compass needle makes with the true north and south line—the angle known to the man of science as the magnetic declination, to the mariner and surveyor as the “variation of the compass.”

This is memorable as being the first scientific expedition sent out by any country with the specific object of improving existing

THE EARTH'S MAGNETISM

knowledge regarding certain facts of the Earth's magnetism. Not until somewhat over two centuries later did it occur again, that a sailing ship traversed the oceans with the chief purpose of making magnetic observations.* In July, 1905, there sailed from the port of San Francisco, California, a chartered sailing yacht, the *Galilee*, sent under the auspices of the Carnegie Institution of Washington, on the sole mission to determine the magnetic elements at sea, for the benefit of both the mariner and the man of science, as was also the purpose of Halley's voyages. Four years later, in 1909, a specially built non-magnetic vessel, likewise under the auspices of the Carnegie Institution of Washington, left New York for St. Johns, Newfoundland, and thence proceeded to Falmouth, along practically the same track followed by Halley's ship. Since then this vessel, the *Carnegie*, has circumnavigated the globe and has repeatedly intersected the course of the *Paramour Pink* in the Atlantic Ocean.

In view of the historic interest thus attaching to Halley's magnetic expedition, it will be well worth our while to use this as our starting point or centre, the fifth point in the Chinese compass. The instructions given Halley, as far as they pertained to his observational work, were as follows :—

“Whereas his Majesty has been pleased to lend his ‘Pink the Paramour’ for your proceeding with her on an expedition to improve the knowledge of the Longitude and variations of the Compasse, which shipp is now completely Man'd, Stored, and Victualled, at his Majesty's charge for the said Expedition : you are therefore hereby required and directed to proceed with her according to the following instructions :—

“You are to make the best of your way to the southward of the Equator, and there to observe on the East Coast of South America, and the West Coast of Africa, the variations of the Compasse with all the accuracy you can, as also the true situation both of Longitude and Latitude of the Ports where you arrive.

“You are likewise to make the like observations at as many of the islands in the seas between the aforesaid Coasts as you can (without too much deviation) bring into your Course ; and, if the season of the year permit, you are to stand soe farr into the South till you discover the Coast of the Terra Incognita, supposed to lie

* Valuable magnetic data have been secured by various expeditions since Halley's time, but either the magnetic work was merely incidental, or formed part of a general scientific programme, or was combined with some geographical object, such as Arctic or Antarctic exploration—the memorable *Erebus* and *Terror* expeditions, for example.

BEDROCK

between Mongolan's Straits and the Cape of Good Hope, which Coast you carefully lay down in its true position. In your return home you are to visit the English West India Plantations or as many of them as conveniently you may, and in them make such observations as may contribute to lay them down truly in their Geographical Situation. And in all the Course of your voyage you must be carefull to omit no opportunity of noting the variation of the Compasse, of which you are to keep a Register in your Journal."

Curiously enough, Halley, though a prominent member of the Royal Society, never contributed a paper to it, nor did he publish anything elsewhere, on these voyages of his, his observations, or resulting conclusions. Not until 1775 were Halley's journal and observations published, and then by Alexander Dalrymple in his *Collection of Voyages chiefly in the Southern Atlantick Ocean*, from the manuscript in the possession of the Board of Longitude at London. Halley appears to have contented himself with laying down the results of his work on a chart entitled "A new and correct Sea Chart of the Whole World, showing the Variations of the Compass as they are found in the year 1700." This chart is often briefly referred to under the title "Tabula Nautica." The first edition, published probably in 1701, covered only the ocean—the Atlantic—traversed by Halley himself; for the later edition, as the chart was now to cover the greater part of the globe, he had to collect and utilise observations made by others. No printed reference to the early edition, either by Halley or by anyone else, prior to my discovery of a copy in the British Museum in 1895, has thus far come to light. Yet this particular chart, termed by me the "Atlantic Chart," to distinguish it from the later one—the "World Chart"—is especially interesting, as it contains the routes followed by the *Paramour Pink*. Airy, when he reproduced Halley's "World Chart" in the Greenwich observations of 1869, was seemingly not aware of the "Atlantic Chart." *

The only description of Halley's chart by himself, thus far found, is that either attached to certain editions of the chart or contained

* Those interested in the history of the Halley charts may be referred to the various articles by L. A. Bauer in *Nature*, May 23rd, 1895, p. 79, and in *Terrestrial Magnetism*, January, 1896, and September, 1913; the last named reference also contains a compilation of the magnetic results obtained on Halley's expedition.

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on an accompanying leaflet. This, however, is very brief and was chiefly intended to instruct mariners in the use of the chart. Halley points out that in certain regions where the "Curves" run suitably, they may be used "to estimate the Longitude at Sea thereby." To his lines of equal "magnetic variation" he gave no distinctive name, simply referring to them as the "Curve Lines." Thus he says: "What is here properly *New*, is the *Curve Lines* drawn over the several Seas, to show the degrees of the *Variation* of the *Magnetical Needle*, or *Sea-Compass*." He does, however, use the term, "Line of No Variation." For some time these lines were referred to by others as the "Halleyan Lines." Hansteen, a century later, introduced the term, "isogonic lines," which is now generally adopted. According to Hellmann there is reason for believing that some attempts had been made before those of Halley to give on a globe or a map a graphical representation of the direction in which a compass needle points. It is conceded, however, that Halley's was the first successful attempt; his "variation chart" was the first magnetic chart based on sufficient observational data to give it immediately both practical and scientific value.*

After the publication of his chart—the most important contribution to the observation-material of terrestrial magnetism at the time—Halley made no further attempt to establish a theory or to improve on his early magnetic speculations. He appears finally to have adopted the view so clearly formulated by Professor Turner †:—

"that the perception of the need for observations, the faith that something will come of them, and the skill and energy to act on that faith—that these qualities all of which are possessed by any observer worthy the name, have at least as much to do with the advance of Science as the formulation of a theory, even of a correct theory."

We find Halley embracing every occasion

"to recommend to all Masters of Ships, and all others, Lovers

* Mountaine and Dodson, the authors of the second and revised edition (1744) of the Halley Chart, and of the third (1756), published in connection with the latter a small tract: "*An Account of the Methods used to describe Lines on Dr. Halley's Chart of the terraqueous Globe, showing the variation of the magnetic needle about the year 1756 in all the known seas . . .*" London, 1758, 4°." This tract was again published in 1784.

† Pres. Address, Sec. A., Brit. Assoc. Adv. Sci., 1911.

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of Natural Truths, that they use their utmost Diligence to make, or procure to be made, Observations of these Variations in all parts of the World, and that they please to communicate them to the *Royal Society*, in order to leave as compleat a History as may be to those that are hereafter to compare all together, and to compleat and perfect this abstruse Theory."

Consulting the minutes of the Royal Society, it is found that Halley communicated, from time to time, the results of magnetic observations received from various expeditions, as also the values of the magnetic declination observed by himself, at London, viz. :—

"1701, May 7.—Mr. Halley tried the experiment of the Variation of the Needle this day, with the two needles he had with him in his late Voyage : and by the one the Variation was $7^{\circ} 40'$, by the other $8^{\circ} 00' W.$

"1702, July 8.—Mr. Halley observed the Variation of the Needle, which was found to be $8\frac{1}{2}^{\circ}$ Westward, or very near it.

"1716, May 24.—Dr. Halley reported that he had drawn a Meridian Line on the stone erected in the Society's yard before the repository and that the Variation was found at present to be full twelve degrees."

These observations of the magnetic declination of 1701, 1702, and 1716 are perhaps printed here for the first time and are not found in any of the compilations of magnetic declinations at London published thus far. Only Halley's earlier observations, namely, those of 1672 ($2^{\circ} 30' W.$), 1683 ($4^{\circ} 30' W.$), and of 1692 ($6^{\circ} 00' W.$), having been given by Halley himself in his printed papers of 1683 and 1692, have become known to compilers.

CHANGE OF THE MAGNETIC DECLINATION IN THE ATLANTIC OCEAN SINCE HALLEY'S CHART.

In view of the fact that the two vessels—the *Paramour Pink* and the *Carnegie*—both being primarily dependent for their motive power upon the prevailing winds in the Atlantic Ocean, have followed nearly identical courses, it will be a matter of no little interest to compare the values of the magnetic declination given on Halley's chart for 1700 with those obtained by the *Carnegie* in her cruises of 1909—10. We find first that over the entire Atlantic, from $50^{\circ} N.$ to $40^{\circ} S.$, the north end of the compass needle in 1910 was to the west of the compass direction of 1700, by amounts varying with locality. Thus for various important ports the

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approximate change was as follows: New York, $2^{\circ}9$ W.; St. Johns, Newfoundland, $14^{\circ}6$ W.; Falmouth, England, $10^{\circ}4$ W.; Funchal, Madeira, $15^{\circ}6$ W.; Bermuda, $10^{\circ}5$ W.; Porto Rico, $7^{\circ}6$ W.; Para, Brazil, $14^{\circ}6$ W.; Rio de Janeiro, $20^{\circ}8$ W.; Buenos Aires, $13^{\circ}0$ W.; Cape Town, $16^{\circ}2$ W.

If we follow a line passing through the points of maximum change in the Atlantic Ocean, we find for the following points:—

VALUES OF THE MAGNETIC DECLINATION IN 1700 AND 1910.

Latitude.	Longitude.	<i>Paramour Pink</i> , 1700.	<i>Carnegie</i> , 1910.	Secular Change (1910—1700).
$50^{\circ}4$ N.	$30^{\circ}4$ W.	$11^{\circ}3$ W.	$29^{\circ}5$ W.	$18^{\circ}2$ W.
$35^{\circ}9$ N.	$47^{\circ}0$ W.	$4^{\circ}0$ W.	$22^{\circ}1$ W.	$18^{\circ}1$ W.
$21^{\circ}0$ N.	$30^{\circ}9$ W.	$0^{\circ}6$ W.	$19^{\circ}2$ W.	$18^{\circ}6$ W.
$5^{\circ}9$ N.	$35^{\circ}8$ W.	$2^{\circ}5$ E.	$16^{\circ}5$ W.	$19^{\circ}0$ W.
$40^{\circ}6$ S.	$25^{\circ}2$ W.	$10^{\circ}7$ E.	$17^{\circ}5$ W.	$28^{\circ}2$ W.

We see, accordingly, that the compass direction, in the course of time, suffers large changes; for the region and time-interval considered the changes vary from about 3° off New York to 28° in the Atlantic Ocean about midway between Buenos Aires and Cape Town. Even these amounts may not represent the total or maximum change during the period in question.

Equally to be noted with these large changes with time is the important fact that the *amount of change* is as dependent upon locality as is the prevailing compass direction itself, which for over four centuries has been known to be anything but "true to the pole."

We have thus had impressed upon us this important fact: Two sailing vessels cruising in the Atlantic Ocean from port to port—the one in 1700 and the other in 1910—were forced by the prevailing winds to follow very closely identical courses. If, however, these two vessels had been directed to follow certain definite magnetic courses, and if we may suppose that they had such motive power as to render them independent of the winds, then their respective paths would have diverged considerably. For example, if the *Carnegie* had set out from St. Johns, Newfoundland, to follow the same magnetic courses as those of the *Paramour Pink*, instead of

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coming to anchor in Falmouth Harbour, she would have made a land-fall somewhere on the north-west coast of Scotland. In brief, while the sailing directions as governed by the winds over the Atlantic Ocean are the same now as they were during Halley's time, the magnetic directions or bearings of the compass that a vessel must follow to reach a given port have greatly altered. To quote from the suggestive essay on Terrestrial Magnetism by John F. W. Herschel : *

" The configuration of our globe—the distribution of temperature in its interior—the tides and currents of the ocean—the general course of winds and the affections of climate—whatever slow changes may be induced in them by those revolutions which geology traces—yet remain for thousands of years appreciably constant. The monsoon, which favours or opposes the progress of the steamer along the Red Sea, is the same which wafted to and fro the ships of Solomon. Eternal snows occupy the same regions, and whiten the same mountains—and springs well forth at the same elevated temperature, from the same sources, now as in the earliest recorded history. But the magnetic state of our globe is one of swift and ceaseless change. A few years suffice to alter materially, and the lapse of half a century or a century to obliterate and completely remodel the form and situation of those lines on its surface, which geometers have supposed to be drawn, in order to give a general and graphical view of the direction and intensity of the magnetic forces at any given epoch."

REGARDING LONGITUDE DETERMINATIONS AT SEA.

One important result of Halley's voyage and of the publication of his chart was the awakening of renewed interest in the improvement of methods for determining the longitude at sea. Recalling Halley's instructions, we note that one of the objects of his expedition was " to improve the knowledge of the Longitude."

When the discovery was made that the magnetic declination varied from place to place, the idea immediately occurred to Columbus, as also to Cabot, that the longitude might be determined at sea by means of this fact. Antonio Pigafetta, who accompanied Magellan on his first voyage around the world in 1522, definitely proposed, in his book on navigation, this method of longitude determination. The line of no magnetic declination, which at that

* *Essays from the Edinburgh and Quarterly Reviews, with Addresses and Other Pieces*, by Sir John F. W. Herschel, London, 1857, pp. 69—70.

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time passed through the Azores, was regarded as the natural meridian from which to count longitude. When later it was found, as was first remarked by J. de Acosta in his *Historia Natural* . . . *Sevilla*, 1590, that there were four such lines, it was again thought that these quadrantal divisions could be utilised for reckoning longitudes. In 1674 Charles II. appointed a commission to examine into the pretensions of a scheme devised by Henry Bond for ascertaining the longitude by the "variation of the compass."

Halley's chart, however, definitely showed that it would be, in general, futile to attempt to determine the longitude by means of an element so variable and so irregular in its distribution as is the magnetic declination. Nevertheless, the hope that some magnetic phenomenon might yet serve to aid in the solution of this problem did not die immediately.

In 1721 we find William Whiston, Newton's successor at Cambridge, installing dip circles on a number of vessels, with instructions to observe diligently the magnetic dip in order to determine whether by means of this element the longitude could be better found at sea than by the magnetic declination; he likewise hoped thus to determine the latitude at sea.

It is also interesting here to note that when Dr. Johnson was at Oxford, he gave in 1756 to the Bodleian Library a thin quarto of twenty-one pages, entitled *An Account of an Attempt to ascertain the Longitude at Sea by an exact Theory of the Variation of the Magnetical Needle, etc.*, by Zachariah Williams, published at London in 1755; Johnson entered it with his own hand in the Library Catalogue. Boswell relates that Johnson himself wrote the English version for Williams, and, in order to make it more extensively known, also had an Italian translation prepared by his friend, Signor Baretti.

For fully three centuries the idea that the longitude could be determined at sea with the aid of some magnetic element, though proved to be fallacious, served a most useful purpose by furnishing the necessary incentive to observe the magnetic elements. This is a striking illustration of the soundness of the position taken by Maxwell when he said: "I never try to dissuade a man from trying an experiment; if he does not find what he wants, he may find out something else." It was indeed true of these magnetic longitude

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seekers, that they failed in their purpose, but they contributed data of inestimable value to the advancement of our knowledge of the Earth's magnetism.

Before leaving this subject it might be said that Halley himself proposed an astronomical method for solving the longitude problem and, with Newton, he was responsible for the Act of 1714 offering a reward to any person who should devise a satisfactory method for the determination of the longitude at sea. He also improved some of the instruments used in navigation.

Another result of Halley's various voyages deserves mention here, though not immediately concerned with the subject of our lecture, namely, his theory of the cause of the trade winds.* On certain editions of his "Variation Chart," there was given, in addition to the lines of equal magnetic variation, a "View of the Generall and Coasting Trade Winds and Monsoons or shifting Trade Winds."

COMPLEXITY OF THE EARTH'S MAGNETISM.

Reference has already been made to Halley's attempts, before his magnetic expedition, to establish a theory respecting the phenomena of the compass needle. Thus in 1683 he published in the *Philosophical Transactions* of the Royal Society "A Theory of the Variation of the Magnetical Compass," and in 1692, in the same *Transactions*, "An Account of the Cause of the Change of the Variation of the Magnetic Needle."

In these papers Halley rejected the hypothesis which had been accepted up to that time and on the basis of which elaborate tables of the magnetic declination had been constructed by previous investigators, namely, that the directions assumed by a compass needle in various parts of the Earth could be accounted for by a simple magnetisation parallel to a diameter so that the magnetic poles would be diametrically opposite to each other. While the conclusion reached by him that "the whole Globe of the Earth is one great Magnet having four Magnetical Poles, or Points of Attraction, near each Pole of the Equator Two," has, in a certain sense, been found to be incorrect, nevertheless, this view appears to have been the first definite recognition of the heterogeneity or complexity of the Earth's magnetic condition.

* See *Miscellanea Curiosa*, Vol. I., pp. 61—80, and Pl. 2.

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The increased knowledge gained from magnetic surveys since Halley's time has taught that the more carefully a country has been explored, *i.e.*, the nearer together the points at which the magnetic elements have been determined, the greater is the number of irregularities usually shown by the so-called isomagnetic lines ; indeed, regions have been found where no system of lines can adequately and correctly represent the prevailing magnetic conditions. We have learned that the regularities in the distribution of the Earth's magnetism, far from being normal features, as was once thought, are, instead, the abnormal ones, and that the irregularities are the normal and to-be-expected phenomena.

The magnetic forces, as measured at any given point on the Earth's surface, appear, according to various analyses, to be the resultant effects of : (1) a general or terrestrial magnetic field due to the general magnetic condition of the whole Earth ; (2) a general terrestrial disturbing cause which distorts at the place of observation the general magnetic condition of the Earth ; (3) a disturbing effect continental in extent ; (4) a regional disturbance effect due to low-lying magnetised substances ; and (5) a local disturbance due to the magnetised masses in the immediate vicinity.

No formula has as yet been established which will represent the observational facts within the error of observation, in fact not even with sufficient accuracy for the practical purposes of the surveyor and of the mariner.

THE EARTH'S MAGNETIC POLES.

We have noticed that Halley, as the result of his study of the observations of the magnetic declination, as far as they had become known up to 1683, reached the conclusion that the Earth had " four Magnetical Poles, or Points of Attraction." Some confusion has arisen as to the precise meaning which Halley attached to his " Poles." Owing to his alternative term—" Points of Attraction "—certain eminent writers have sought to identify Halley's supposed four Magnetic Poles with the four *foci* of maximum total magnetic force, whose existence appeared to be indicated when, near the middle of the nineteenth century, it became possible to construct a chart of the lines of equal magnetic force. By this incorrect inference these authors have unwittingly credited Halley with a discovery which,

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in the absence at the time of any observation whatsoever respecting the strength of the Earth's magnetic force, he could not possibly have made. The real merit and purport of Halley's deduction has thereby been obscured. The observation-material at Halley's disposal, before he himself enriched the material during his voyages, consisted of some miscellaneous observations of the compass direction and a few values of the magnetic dip. As has been said, there were no observations of the magnetic force, for the art of measuring this element had not yet become known.

Scrutinising carefully his scanty observation-material, Halley noticed that the direction of the compass needle did not change from place to place in the simple way it would if, for example, the Earth had two Magnetic Poles diametrically opposite each other. In the latter case, the needle would set itself tangent to the great circle passing through the Magnetic Poles and the place of observation. If, then, the compass direction were known at two places, sufficiently far apart, the points of intersection of the two great circles drawn respectively tangent to these compass directions would be the two diametrically opposite Magnetic Poles. It is such points of intersection—"points of convergence," as Hansteen later called them—which Halley had in mind as "Magnetic Poles." He was the first to perceive clearly the fact—abundantly verified since—that the various points of convergence as found from successive pairs of compass directions, in the manner just described, do not fall together as they should on the basis of a simple or regular magnetisation of the Earth. However, it appeared to Halley, and the same conclusion was reached over 100 years later by the illustrious Norwegian magnetician, Hansteen, that the several points of convergence grouped themselves, in a general way, about two main centres,

"near each Pole of the Equator Two; and that, in those parts of the World which lie near adjacent to any one of those Magnetical Poles, the Needle is govern'd thereby, the nearest Pole being always predominant over the more remote."

It will not be well to lay greater stress upon this deduction, nor upon those in his 1692 paper, where he seeks to account for the existence of his four "Magnetic Poles" and for the secular variation, than to say that Halley drew the best possible conclusions the material at his disposal permitted. In fact, his conclusions were not materially

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improved upon until a century and a half later, when a much more complete knowledge of the distribution of the Earth's magnetism had been gained, and when the various mathematical attempts which had been made to compute the magnetic elements, on the basis of more or less intricate hypotheses as to the Earth's magnetisation, had been found to be inadequate. Some later investigators, indeed, might have spared themselves considerable pains had they previously familiarised themselves more thoroughly with Halley's work.

When we to-day speak of the Earth's Magnetic Poles, it is generally recognised that those points on the Earth's surface are meant where the dipping needle stands precisely vertical and where the magnetic dip is, accordingly, 90° . This definition permits, with the aid of the dipping needle, of a precise determination of the Magnetic Poles, though, of course, it must not be understood that these Poles are mathematical points; the area over which the dip may be found to be 90° , within the instrumental means of determination, may, in fact, be several miles square. A more or less extensive magnetic survey of the region round about would be required to eliminate the possibility of disturbing influences owing to local deposits of iron-ore. At these "Poles," since the magnetic force exerted by the Earth is all up and down, with no side component, a compass needle would have no directive force acting upon it. Some distance before reaching the Magnetic Pole it would become sluggish, and directly over the Pole itself it would be of no more use than a brass needle to indicate any definite direction.

Excluding for the present the purely "local magnetic poles" caused by extraordinary local deposits of attracting masses, all observations to date show that there are but two such points (or areas) where the dipping needle stands vertical, one in the Northern Hemisphere, located by Captain James Clark Ross, in June, 1831, in latitude $70^\circ 1'$ N. and longitude $96^\circ 8'$ W.* and the other in the Southern Hemisphere, lying, according to the observations of the recent Antarctic expeditions, about in latitude $72^\circ 7'$ S. and

* During Captain Amundsen's completion of the *Northwest Passage*, 1903—07, he also made observations with a view to locating the North Magnetic Pole, but the resulting position has not yet been published.

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longitude 156° E. The Magnetic Poles, therefore, are, on the average, about 1,200 miles from the geographical poles. Owing to the asymmetrical distribution of the Earth's magnetism, the Magnetic Poles are not diametrically opposite each other, even if the positions given applied to the same year ; in fact, the perpendicular distance from the Earth's centre to the chord connecting the Magnetic Poles is about 750 miles.

Let us suppose now that one explorer starts out from Oxford, where the compass points at present about 16° west, and follows always the direction shown by the north end of the compass needle, whereas another starts north from Washington, where the compass bears about 5° west, and follows likewise the direction of the compass needle. The paths thus traced out by them are the so-called "magnetic meridians," which, owing to the irregular way in which the Earth is magnetised, would not be straight lines or arcs of great circles, but more or less devious lines. Could these magnetic meridians be followed into the Arctic regions, they would be found to intersect at the North Magnetic Pole.

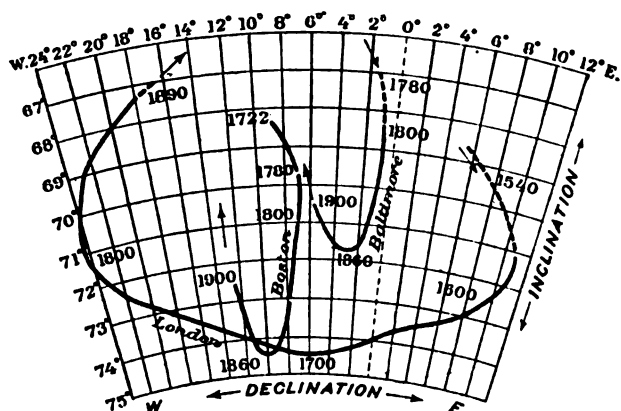
Owing to the irregular distribution of the Earth's magnetism, the points of greatest intensity of the total magnetic force depart widely in their locations from the Magnetic Poles. Thus there are in the Northern Hemisphere two distinct maxima of total magnetic force, one in the north-east of Siberia and the other in Canada to the south-west, approximately, of Hudson's Bay. A magnetic survey of the latter region is being made this summer by an expedition sent out by the Department of Terrestrial Magnetism.

DO THE MAGNETIC POLES MOVE ?

Possibly the most frequent question asked of those engaged in magnetic work is : " Do the Magnetic Poles move with the lapse of years, and if so, why ? " Unfortunately, as has already been shown, there are no direct observations as yet on which to base a definite statement. But it would be singular, indeed, if these points remained fixed and were not affected by fluctuations such as are now known from three centuries of observations to exist in every one of the Earth's magnetic phenomena. It is quite possible, in fact, that the Magnetic Poles pass through certain motions even in the course of a day or suffer displacements during magnetic storms.

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The diagram below shows the changes in the direction of the compass (magnetic declination), as well as in the direction of the dip needle (magnetic inclination), as far as known, for London, Baltimore, and Boston. Imagine yourself, if you will, standing at the centre of a great magnetised needle so suspended as to be free to assume the direction actually taken by the lines of magnetic force at the place of observation, and let us suppose you are looking towards the north-pointing end of the needle. Could you gaze long enough, you would see a curve described in space by the observed end of the needle. This curve would lie on a sphere whose radius



Curves showing the Secular Change in the Magnetic Declination and in the Dip at London, Boston, and Baltimore. [Drawn for supposed length of freely suspended magnetic needle of about 50 centimetres, or nearly 20 inches.]

is the half-length of the suspended needle and for graphical representation we may take a central projection of it on a plane tangent to the sphere at about the middle point of the curve. The curves here given were constructed by me with the aid of the accumulated observations up to about 1895; the course followed by the needle since 1895 will be discussed later.

A number of interesting and instructive facts follow from these curves; time will permit us to give our attention only to the chief ones. It is seen that at London, for example, the compass reached its maximum easterly direction of about 11° in the year 1580, hence during the middle of Queen Elizabeth's reign; thereafter the easterly direction began to diminish until about 1658, the year of

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Cromwell's death, when the needle bore due north and then swung over to the west, continuing to do so until it reached a maximum westerly direction of somewhat over 24° in about 1812. Hence in the interval of about 232 years (1850—1812) the compass direction changed at London from 11° E. to 24° W., or 35° . At the present time it points about $15\frac{1}{2}^\circ$ W., or nearly 9° less than in 1812, and a most interesting question doubtless immediately occurs to all of us: Will the freely suspended magnetic needle ever return precisely to a direction taken at some previous time or is there any definite cycle of changes which will repeat itself from time to time?

Here again no wholly definite answer can be given, primarily because of the fact, as will be seen from the diagram, that, if there be such a cycle, it embraces many more years than are covered thus far by the interval of observation. For some European stations, *e.g.*, Paris and Rome, the observation-interval is somewhat longer than at London, but still not long enough for definite prediction as to the future course of the magnetic needle.

The diagram shows also that in the United States the changes in the compass direction, as far back as they are known, have not been as great as those during the same time at London. Thus, at Baltimore, for example, the compass appears to have reached a maximum westerly amount of about $6^\circ.1$ near 1670, and a minimum of $\frac{2}{3}^\circ$ in 1802, after which, instead of passing through a zero value as at London in 1658, and swinging to the eastward, it turned back and began to increase its westerly direction until at the present time the amount is about $6\frac{1}{2}^\circ$. Thus, at this station the compass direction passed from a maximum to a minimum in about 132 years and the total change was but $5\frac{1}{2}^\circ$, or only one-sixth to one-seventh of that at London.

In brief, the facts revealed by the known compass changes in my country cannot be brought in harmony with those witnessed in your country, unless we assume that the length of the cycle of complete change is many times longer than merely twice the period between a maximum and a minimum bearing of the compass. There are evidences, furthermore, into which we cannot go here, to indicate that the cycle of change at one station is not of the type which would result were we to close the apparently nearly completed curve at London by uniting the two ends in some simple manner.

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On the contrary, the evidences point to cycles within cycles and to the probability that the secular variation curve, instead of being a single closed curve, may consist of smaller loops within a larger one, etc.; it is even questionable whether there ever will be exact closure of the curve.

There is at present another matter of no little interest with regard to England which should be pointed out here. It will be seen from the London curve that the dip of the needle below the horizon reached its maximum amount of $74^{\circ}4$ in about 1688. At this time the compass changed its direction the maximum amount of $13'$ per year. The curve would seem to indicate that the time of a minimum dip is now approaching; this phase has already occurred at Pawlowsk and seems to be now taking place at Potsdam and is travelling westward. Whether it will reach London and when cannot be answered definitely. However, it is a matter of no little interest, in this connection, to observe that the annual amount of change in the compass direction has in recent years received a remarkable acceleration in this part of the Earth. Thus, as is shown by the magnetic observatory records, it has almost steadily risen from $4'$ per year in 1902 to about $9'$ per year in 1912! Whether this portends an early approach of the phase of minimum dip at London is one of the many interesting questions continually arising respecting the perplexing phenomena of the Earth's magnetism. The course of the needle since 1890 has been about as shown by the arrow; thus in 1910 the magnetic declination was approximately $15^{\circ}9$ W. and the dip was $66^{\circ}9$.

One thing more. Note that for each of the three curves as far as drawn, the motion of the freely suspended magnetic needle has been clockwise, i.e., the same as the motion of the hands of a watch. This fact, as shown by the curves in other parts of the world, constructed with the aid of the available observations, appears to hold generally in both the Northern and Southern Hemispheres, except for certain retrograde motions which thus far have not been of the same extent as the direct one, although, of course, it is not affirmed that they may not become so later. Such retrograde motions are at present being experienced in certain parts of the United States. Thus, for example, the compass pointed in 1910, $6^{\circ}25$ W. at Baltimore and $13^{\circ}35$ W. at Boston, and in the same year the magnetic

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dip was $70^{\circ}9$ at Baltimore and $73^{\circ}1$ at Boston. If we plot these values on the diagram, we shall find that the curves for Boston and Baltimore, instead of progressing in the direction of the arrows, passed through a secondary crest about 1895 and then bent over to the left; how long this will continue cannot be foretold. [Several slides in illustration of the various facts of the secular change were shown.]

The question as to the cause of the remarkable changes from time to time in the Earth's magnetic condition, as indicated by these curves, has been a fruitful source of speculation since 1634, when Gellibrand definitely proved the fact that the compass direction varies from year to year. Some of the best minds have been engaged with the discovery of the cause, but the riddle is still unsolved. Hence, as regards the actual motions of the Earth's Magnetic Poles and the precise cause or causes, we may still say with Halley that these are "Secrets as yet utterly unknown to Mankind, and are reserv'd for the Industry of future Ages."

A mathematical analysis of the accumulated material shows that, in order to find an adequate explanation of the secular variation of the Earth's magnetism, we must reckon with systems of magnetic or electric forces having their seats both below and above the Earth's crust. There would also appear to be some evidence that, in addition to a motion of the Magnetic Poles or magnetic axes of the Earth, we may also have to take into account a possible diminution in the Earth's magnetic moment or intensity of magnetisation.

THE ORIGIN OF THE EARTH'S MAGNETISM.

Before concluding this lecture, we ought, perhaps, in the few minutes remaining, to say something regarding the status of the ever-recurring question as to the origin of the Earth's magnetism. Assuming that the magnetism of our planet is uniformly distributed throughout its mass, it is found that the average intensity of magnetisation is only about $\frac{1}{10000}$ of very highly magnetised hard steel. Professor Fleming, in his very suggestive popular lecture on the "Earth, a Great Magnet," given at the meeting in 1896 of the British Association for the Advancement of Science, made this statement:—

"Taken as a whole, the Earth is a feeble magnet. If our globe were wholly made of steel and magnetized as highly as an

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ordinary steel-bar magnet, the magnetic forces at its surface would be at least 100 times as great as they are now. That might be an advantage or a very great disadvantage."

If, however, we could penetrate the Earth's crust, we would find at a distance of only about 12 miles a temperature so great that, according to present laboratory facts, all magnetisation would necessarily cease. Hence, if the Earth's magnetic field arises from an actual magnetisation of the substances composing the Earth, these substances must be confined within a comparatively thin shell. But the question immediately arises: Is this argument correct? May it not be that just as the point of liquefaction is raised by increased pressure, so is also the critical temperature of magnetisation. It may thus occur that the effect due to increase of pressure with depth of penetration more than balances that due to increased temperature. There are at present no wholly decisive experiments which may be drawn upon to answer this query.

The hypothesis that the Earth may be an electromagnet also meets with difficulties when we attempt to account for the origin, direction, and maintenance of the required currents. In spite of the accumulated facts of over three centuries, we are still unable to say definitely to what the Earth's magnetic field is really due. Perhaps we may not be able to solve the riddle until the physicist answers for us the questions: What is a magnet? What is magnetism, in general?

In the *Devil's Dictionary* by Ambrose Bierce, published in 1911, the following definitions are given: "Magnet, *n.*—Something acted upon by magnetism. Magnetism, *n.*—Something acting upon a magnet." In explanation the author cynically remarks: "The two definitions immediately foregoing are condensed from the works of 1,000 eminent scientists, who have illuminated the subject with a great white light, to the inexpressible advancement of human knowledge." *

* These definitions and accompanying remarks may have had their origin in the following interesting anecdote told in the *American Review of Reviews* for August, 1909, of the late Professor Simon Newcomb, by Mr. A. E. Bostwick, associate editor of the *Standard Dictionary*. Of the definitions in physical science for this dictionary, Newcomb had general oversight, and on one occasion he took exception to the definitions framed for the words "magnet" and "magnetism," as based, in the absence of authoritative knowledge of the causes, simply upon the properties manifested by the things. After

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A line of thought first suggested by Schuster and Lord Kelvin, that every large rotating mass, due to an as yet undiscovered cause, may be a magnet, should be considered in conclusion, though we may do so but briefly. If this be true, then magnetism is not confined to our planet alone, but all celestial bodies are surrounded by magnetic fields. Thus far no laboratory experiment, possibly owing to lack of required sensitiveness in the measuring instruments, has detected any magnetic field arising solely from rotation. Schuster and Swann have recently discussed the character and magnitude of the effects from the possible causes which may operate if the Earth's magnetic field be related in some manner to its rotation.

In 1900—03 Sutherland propounded a theory for the origin of the Earth's magnetism, which, briefly stated, is this : We know that electricity is an essential constituent of matter and that in every atom, if it be electrically neutral, there are equal amounts of negative and positive electricity. So with the whole Earth. Since it is almost electrically neutral, suppose that the total negative charge, while practically equal to the total positive one, occupies a slightly different volume from that of the positive charge, or, in brief, that the volume densities of the two body charges differ slightly. Then, because of the rotation of the electric charges with the Earth, a magnetic field arises. I have recently repeated Sutherland's calculations and, as I had previously found that the Earth's intensity of magnetisation increased systematically towards the equator, I have included a term to represent a possible effect similar in its distribution to that arising were the Earth's centrifugal force the operating cause. The computations show that to satisfy the known phenomena of the Earth's magnetism, the volume density of the negative charge must be smaller than that of the positive, or, in other words, the Earth's total negative charge must be distributed through the larger sphere, and, if that be the whole Earth itself, then for the chief term involved in the magnetic potential, the surface of the sphere containing the positive charge need be, on the

writing and erasing alternately for an hour or more, he finally confessed, however, with a hearty laugh, that he himself could offer nothing better than the following pair of definitions : *Magnet*, a body capable of exerting magnetic force, and *Magnetic Force*, the force exerted by a magnet.

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average, only 0.4×10^{-8} cms., i.e. four-tenths of the radius of an ordinary molecule, below that of the Earth's surface to give a magnetic field of the required strength. Taking the average atomic weight of the Earth's substance in round numbers as 50, the mean volume density of either charge would be about 3.3×10^{12} electrostatic units.

At present there is little hope that a magnetic field, caused just as supposed, can be detected in the laboratory. For a sphere of 15 cms. radius, rotating 100 times a second, the magnetic intensity at the poles would be but one hundred-millionth part (10^{-8}) of that of the Earth. We thus see that the quantities involved in the solution of one of the great problems confronting the student of the Earth's physics—the origin of the Earth's magnetic field—may be of such a minute order as to be beyond the ken at present of the laboratory experimentalist. Perhaps the effects become appreciable in the case of the Earth because of the fortunate fact that it is a body of sufficient size and angular velocity.

On the other hand, the geophysicist is at a great disadvantage in that he is unable to bring his Earth-magnet into the laboratory and to experiment upon it—to reverse the direction of rotation, for example, and see what would happen! Fortunately for him, however, Nature comes to his relief somewhat and performs experiments for him on his great magnet on a world-wide scale, by producing in an incredibly short time, manifold, and at times startling variations and fluctuations in the apparently fixed magnetisation of the Earth. Thus, on September 25th, 1909, there occurred the most remarkable magnetic storm on record, during which, within a few minutes, the Earth's magnetic moment, or intensity of magnetisation, was altered by about one-twentieth to one-thirtieth part. The Earth's magnetic condition was below par for fully three months thereafter. As this severe storm was accompanied by a brilliant display of polar lights, this is the most appropriate place to recall that Halley made the first suggestion of a connection between the *aurora borealis* and the Earth's magnetism.

It is firmly believed that a long step forward will have been taken toward the discovery of the origin of the Earth's magnetism when once we have found out what causes it to vary in the surprising manner shown by the secular or long-period changes, by the magnetic

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storms, and by the numerous other fluctuations, such as the diurnal variation, for example. The keynote of modern investigation in terrestrial magnetism, as in the biological sciences, must surely be the study of the variations and mutations !

Is it not probable that the very features of the Earth's magnetism regarded at one time as defects—the "constant inconstancies," as an early writer quaintly put it—will instead become sources of help and inspiration from totally different points of view or in some entirely different line of thought ? Who knows of what import the riddles of the Earth's magnetism, characterised by eminent physicists as being, next to gravity, the most puzzling of natural forces, may be, not simply to the magnetician alone, but to all interested in the steady progress of the physical sciences ? Thus Schuster suggests that "atmospheric electricity and terrestrial magnetism, treated too long as isolated phenomena, may give us hints on hitherto unknown properties of matter." "The field of investigation into which we are introduced," says Maxwell, "by the study of terrestrial magnetism, is as profound as it is extensive." And, says Sabine, one of England's greatest and most enthusiastic magneticians : "Viewed in itself and its various relations, the magnetism of the Earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit."

MIMICRY AND THE INHERITANCE OF SMALL VARIATIONS

By Professor E. B. Poulton, F.R.S.

THE inheritance of small variations is an issue of such supreme importance that it would perhaps be well to devote the whole of the present article to its consideration. I will, at any rate, put it in the forefront, beginning by the quotation of two passages in which Darwin summed up the labour and the thought of half a lifetime.

“Any variation which is not inherited is unimportant for us. But the number and diversity of inheritable deviations of structure, both those of slight and those of considerable physiological importance, is endless. . . . No breeder doubts how strong is the tendency to inheritance : like produces like is his fundamental belief : doubts have been thrown on this principle by theoretical writers alone. . . . If strange and rare deviations of structure are truly inherited, less strange and commoner deviations may be freely admitted to be inheritable. Perhaps the correct way of viewing the whole subject, would be, to look at the inheritance of every character whatever as the rule, and non-inheritance as the anomaly.”*

“When a new character arises, whatever its nature may be, it generally tends to be inherited, at least in a temporary and sometimes in a most persistent manner. What can be more wonderful than that some trifling peculiarity, not primordially attached to the species, should be transmitted. . . .

“Some writers, who have not attended to natural history, have attempted to show that the force of inheritance has been much exaggerated. The breeders of animals would smile at such simplicity, . . .”†

Why should Professor Bateson, Professor Punnett and their followers make assertions which imply that Darwin was a hasty generaliser, that his opinions on fundamental questions were

* *Origin of Species*, 1st. ed., 1859, pp. 12, 13.

† *Variation of Animals and Plants under Domestication*, Vol. I., 1875, p. 446. The same conclusion is re-stated and examples given on pp. 447 and 449.

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ill-considered and of no importance ? The answer is a simple one. These clever and ingenious men, dazzled and confused by a re-discovery of the utmost interest, and the exciting investigations to which it has led, have entirely lost all perspective and all sense of proportion. Their distorted vision is most unfortunate for English biological science, because young and vigorous naturalists, ever eager in the pursuit of some new thing, are being led into the same hopeless confusion which has overwhelmed their leaders. It is well that the danger should be seen and guarded against, that men should know how far these writers are to be taken seriously when they wander, as they are so apt to do, beyond the details of their own researches. The irresponsibility of de Vries's principal exponent in this country is manifest in the following passages quoted from the same volume* and only eleven pages apart :—

De Vries according to Bateson.

"First we must, as de Vries has shown, distinguish real, genetic variation from *fluctuational* variations, due to environmental and other accidents, which cannot be transmitted" (p. 95).

De Vries according to De Vries.

"Thus we see that the theory of the origin of species by means of natural selection is quite independent of the question, how the variations to be selected arise. They may arise slowly, from simple variations, or suddenly, by mutations; in both cases natural selection will take hold of them, will multiply them if they are beneficial, and in the course of time accumulate them, so as to produce that great diversity of organic life, which we so highly admire" (pp. 83, 84).

An even sharper contrast is evident between the following passages from the same authors. First, Professor Bateson :—

"For the first time he [de Vries] pointed out the clear distinction between the impermanent and non-transmissible variations which he speaks of as *fluctuations*, and the permanent and transmissible variations which he calls *mutations*."†

Now Professor de Vries himself :—

"Sugar beets afford the finest example of the process of artificial

* *Darwin and Modern Science*, Cambridge, 1909.

† *Mendel's Principles of Heredity*, Cambridge, 1909, p. 287.

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selection. In no other plant under cultivation has the technique of selection reached so high a pitch of perfection ; in no other is the method so sure or the result so certain. There is now no sale for beet seed which has not been the result of careful selection.

"Experiments in the selection of sugar capacity began about 1850. This instance shows best, therefore, what can be achieved within half a century by continued selection in one and the same direction, hand in hand with continual improvement of method.

"Progress has been enormous: the average content of the common beet, which at first was a matter of 7—8 %, is now double that amount. Shape, size, and weight, the character of the leaves and especially the reduction in woody tissues have all been the object of selection, and have made the beet much more valuable from the industrial point of view.

"All this has been done by selection of the best individuals afforded by ordinary fluctuating variation. Neither spontaneous variations nor crossings have played any part in it. We are dealing here with the process in its simplest form."*

It is surely the irony of fate that I, who, admiring de Vries as an investigator, think nothing of his contributions to the evolution theory, regarding them as in part already to be found in Darwin and Galton and, when original, puerile—that I should have to correct his professed supporters and exponents, and explain the meaning of a de Vriesian "fluctuation" as contrasted with his "mutation." The difference is *not* that the first are non-transmissible and the second hereditary, but that fluctuations are liable to regression and more and more liable to it the further they have been advanced in any direction by means of selection. Mutations, on the other hand, are a leap to a new position of genetic stability. In all these conceptions de Vries is merely following Galton, who earlier expressed the same conclusions far more clearly and with a much better terminology. I must add that a perfectly correct account of de Vries' conclusions has been given by A. A. W. Hubrecht,† C. B. Davenport,‡ R. H. Lock§ and J. Arthur Thomson.||

* *Die Mutationstheorie*, English Translation by Farmer and Darbishire, Vol. I., 1910, pp. 99, 100.

† *Popular Science Monthly*, July, 1904, p. 205; *Contemporary Review*, No. V., 1908.

‡ *Fifty Years of Darwinism*, New York, 1909, pp. 173—4.

§ *Variation, Heredity and Evolution*, 2nd ed., London, 1909, pp. 75, 135—6, 155.

|| *Heredity*, London, 1908, pp. 78, 98. Quotations from all these writers,

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In spite of de Vries himself, in spite of all that the above-named writers have said, a "de Vriesian mutation," which is the same thing as Galton's better-named "transilient variation," is rapidly being replaced by a "Batesonian mutation," which is the same thing as Weismann's better-named "blastogenic variation"—also called by various writers at various times *constitutional*, *congenital*, *centrifugal*, *genetic*, *inborn*, *innate* and *inherent*. It is a formidable list, and the only objections to adding mutation to it are that it is the least descriptive, the most recently applied, and, because of its history, by far the most confusing term of a list that is already quite long enough.

Mighty is the force of fashion, in science as in other departments of human activity. Even my friend the Master of Christ's College, Cambridge, who very nearly a quarter of a century ago laboured with Dr. Schönland and with me to make known Weismann's conclusions on heredity, even he now hands over to de Vries what de Vries himself has never claimed, viz., Weismann's clear distinction between blastogenic and somatogenic characters.* So also Clifford Dobell, in a passage with a wording which goes far to suggest Weismann's term blastogenic, prefers to perpetuate this cause of confusion, error, and injustice:—

"By *mutation* . . . I mean a permanent change—however small it may be—which takes place in a bacterium and is then transmitted to subsequent generations. The word does not imply anything concerning the magnitude of the change, its suddenness, or the manner of its acquisition. The term denotes a change in genetic constitution. All other changes which are impermanent—depending generally upon changes of the environment—and not hereditarily fixed, are called *modifications*."†

The word mutation—originally introduced by Waagen, used in a different sense by de Vries, used in a third sense erroneously ascribed to de Vries by Bateson—is bringing "confusion worse confounded" upon biological thought. In the interests of clear thinking I cannot help regretting this unnecessary result, although the spread of the word in the Batesonian sense places me in the

as well as a fuller discussion of the unfortunate confusion into which the subject has been thrown, will be found in the author's *Darwin and the Origin*, London, 1909, pp. xi.—xiii., 48—51, 258—280.

* Presidential address to Section D. at Winnipeg, 1909.

† *Journ. Genetics*, Vol. II., No. 4, p. 326.

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happy position of the prophet who sees fulfilment even earlier than he expected.

“A humorist has suggested that the Homer controversy should be settled by a general agreement that the *Iliad* was written not by Homer but by another man with the same name. Those who have heralded with such a flourish of trumpets the profound changes which they assume to be necessary in the Darwinian conception of evolution, may yet save their face by calling the same thing by another name.”*

I now come to details of Professor Punnett's article in the July number of *BEDROCK*, and I here find the irresponsibility already spoken of, the same attitude which seems to be expressed by the words—“Mendelism is so interesting that it really doesn't matter what one says.” Thus, on page 153, speaking of the inheritance of a small variation, we are told that “in no clear case has it been shown to exist.” Does Professor Punnett believe that “family likeness” is hereditary, or that one element in family likeness, such as the shape of a nose or chin, is hereditary; that a voice or trick of movement or expression is hereditary? I do not think that he doubts any of these facts. His statement was just the irresponsible utterance of one who has not thought out the consequences of his own words. And if Professor Punnett still has doubts, at any rate they are not shared by others who are as interested in Mendelian research as he is himself. Thus Professor C. B. Davenport told me in 1909 that he had often been struck with the remarkable persistence of insignificant variations, such as a single small white spot. But leaving the higher animals and coming to butterflies, I had spoken, in the very article† supposed to be criticised by Professor Punnett, of actual evidence in the Hope Collections that “small features in the pattern of the parent [*dardanus*] certainly tend to reappear in her offspring,” and one such feature was described. Professor Punnett never even alludes to the passage. Nor does he refer to Figs. 11 to 14 in Plate III. of my article, distinctly proving that a small variation in pattern exhibited in the female parent was inherited by all her offspring of the same form (*dubia*). I also mentioned on p. 56 that Mr. Lamborn had sent me another much larger family showing the same hereditary persistence of the same small variation. Of course,

* *Darwin and the Origin*, p. 280.

† *BEDROCK*, April, 1913, p. 50.

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it is possible to argue at some length as to what is "small" and what "large," and I will therefore endeavour to avoid all unnecessary discussion by explaining that the "small" variation, described on p. 56 and shown to be hereditary in Plate III., is just such a change as, in my opinion, formed one of the steps by which a mimetic resemblance was attained. Mr. Lamborn also sent to me, in the early part of last year, two magnificent families of *Hypolimnas dinarcha*, together with their female parents. In one parent the white of the hind wing is faintly tinged with yellow, and there is a very slight difference in the pattern of the fore wing. Both these characteristics, as small or smaller than the "steps" I have postulated, strongly tend to be inherited by the female offspring.

It is, furthermore, easy, by a study of the geographical races of almost any wide-ranging species, to supply the evidence Professor Punnett has failed or not troubled to find. The fine work of the Tring Zoological Museum is chiefly devoted to the comparison, description and illustration of these small hereditary differences between races which, by inter-breeding at the margins of their respective areas, are welded together into a single species. I will illustrate this kind of evidence, of which any amount is available, by reference to one of the species mentioned and figured on Plate I., facing p. 151 of Professor Punnett's paper. The figures of *Danaïs chrysippus* clearly show the pattern at the tip of the fore wing of average Indian and Cingalese examples. A small white spot is seen lying opposite the lower end of the white bar on the side turned towards the attachment of the wing. This spot, if it were larger and joined to the bar, would make with it an L-shaped marking. When we follow *D. chrysippus* eastward, for example, into the Macao and Hong Kong districts, that spot does become larger and is sometimes joined to the bar. Following the butterfly westward into Africa, the spot disappears altogether, or, when it persists, is smaller than in Professor Punnett's figures. Other minute geographical changes in the same butterfly might be described, and, as I have said, any number in other species. The only question that remains is their transmissibility, but I imagine that Professor Punnett will hardly doubt that each local pattern of a butterfly, occupying corresponding stations in the different parts of its total habitat, is a hereditary pattern. There is really no room for doubt,

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because geographical races, including some forms of *chrysippus*, have often been bred and found to come true.

Professor Punnett—unintentionally no doubt—misinterprets my views as to the first origin of a mimetic likeness in a butterfly with a pattern widely different from its model. More than once he speaks of the “minute initial variation,” or words to that effect: on p. 153 he alludes to “the difficulty of the initial stages, so clearly recognised by Darwin, and so lightly disposed of by” me: on p. 146 he refers to an interpretation based on the theory of mimicry as “altogether too facile.” I am sorry to indulge in a *tu quoque*, but I must point out that it is very easy to meet the difficulty of the origin of a mimetic likeness by assuming that it appeared in its present form. I have never found it light and simple work to attempt to make out these past histories. The careful study of a long series of specimens from many parts of as wide a range as possible is generally required, and after doing one’s best the dominant feeling at the end is often the desire for more specimens from other localities. If Professor Punnett had troubled to study what I have written he would never have spoken as he does about the supposed “minute initial variation.” I have always recognised that the first variation must be something appreciable, something which, at any rate, at a distance and on the wing would recall the pattern of the model. Mimicry is far more characteristic of forest species than of those living in the open, and Mr. C. F. M. Swynnerton has made the reasonable suggestion that the origin of mimicry is facilitated by the alternating light and shade of a tropical forest, where it is easy to confuse patterns readily distinguishable under ordinary conditions of illumination.

In an earlier article in *BEDROCK** I attempted to trace the origin and history of the mimetic pattern of the eastern female of *Acraea alciope*, which resembles the male of one species of *Planema* and the male and female of another. I gave reasons for the belief that the eastern mimicry was started by the sudden appearance of a white bar crossing the hind wing. I furthermore showed that out of 249 western females bred by Mr. W. A. Lamborn in the Lagos district, a single one exhibited “a well-marked white bar crossing the fore wing,” showing “how a mimetic modification might arise

* No. 1, April, 1912, p. 48.

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if an appropriate model existed in the locality." Concerning the origin of the eastern mimic, the following passage is quoted from p. 63 :—

"It is probable that by spontaneous variation a white band like that shown in Fig. 13 appeared in the ancestral form (Fig. 12), and that this was from the very first sufficient to confer some advantage by suggesting the appearance of a dominant Model (Fig. 6). From this point Natural Selection acting on further variations produced the detailed likeness which we see in the white band itself and in the other mimetic features."

I think Professor Punnett will admit that he has given an unfair impression of my views. But it is not only for the purpose of correcting him that I quote rather fully from the earlier article. In writing it I left off as usual longing for more material. Within the past few months the wish has been gratified. The paper was written after a study of the splendid western series sent to me by my friend, Mr. W. A. Lamborn, Entomologist to the Agricultural Department of Southern Nigeria, and the equally splendid material from Eastern Uganda by my friends, Mr. C. A. Wiggins, D.P.M.O., of the Uganda Protectorate, and Dr. G. D. H. Carpenter, Member of the Royal Society's Sleeping Sickness Commission. The eastern females were nearly all perfect mimics of eastern models, but a very few were of the western type, and of these again a small proportion exhibited the incipient but distinct white bar which suggested the origin of the eastern mimetic form. I was especially anxious for specimens from further west, from a zone of country where I thought the transitional forms might be abundant. Owing to the kindness of my friends Mr. S. A. Neave and Mr. Guy A. K. Marshall, Secretary to the Entomological Research Committee of the Colonial Office, I have now had the chance of studying carefully the fine collection made in Uganda by the former as travelling naturalist to the Committee. Mr. Neave not only collected in Eastern Uganda, with results as regards the female *alciope* similar to those obtained by my other friends, but also travelled westward to the Semliki Valley, the western boundary of Uganda. He here entered the margin of the great tropical forest which stretches unbroken to the west coast. Uganda itself is largely open country with patches of primitive forest, doubtless formerly continuous with one another and with the great forest now ending at the Semliki.

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Mr. Neave collected in the Semliki Valley and in forest patches near it. In his whole collection from this part of Uganda there is not a single mimetic female *alciope* of the eastern type: there are many females of the western type, and of these a considerable proportion bear the incipient bar developed to a very variable extent, and sometimes appearing on the under surface alone. Here, then, in the very zone of country where, on the theory of mimicry, we should expect them to be, we meet with the earliest stage of the eastern mimic, but, so far as we know, never the finished product.

I can hardly expect that this evidence will appeal to Professor Punnett, who seems to be singularly impervious to arguments based on geographical distribution. Thus he makes no reference, save one, to the distribution of the mimetic and other forms of *P. dardanus*, although geographical distribution was the strongest part of the argument he was professing to answer. His one reference hardly strengthens his case. He speaks of "wildly assuming that because a form lives on an island it is therefore ancestral" (p. 164). No assumption was made on such grounds. *Papilio meriones* was held to be ancestral, not because it lives in Madagascar, but because the female possesses the non-mimetic pattern of the male. And Professor Punnett, too, when it suits his purpose, is quite willing to base his arguments on the conclusion that the non-mimetic male of a mimetic species bears the ancestral pattern.* Incidentally it may be remarked that it is somewhat humorous for Professor Punnett to speak of anyone wildly assuming anything.

While we are on the subject of *P. dardanus* it will be convenient to correct another mistaken assumption. Professor Punnett begins a paragraph on p. 161: "Let us . . . for the sake of argument, leave out of account the fact that some, at any rate, of these transitional forms (such as *trimeni*) are specific. . . ." *Trimeni*, named by me in 1906,† is not specific, and has never been spoken of as specific by any writer except Professor Punnett in the above-quoted passage. It is a female form existing side by side with other and mimetic

* See pp. 151, 152, of his BEDROCK paper. It is a pity that in Plate I., facing p. 151, in which Professor Punnett illustrates his argument, *D. chrysippus* should be represented by a male in both outer and inner circles, although all the other species are represented by males in the outer and females in the inner circle.

† *Trans. Ent. Soc., Lond.*, 1906, p. 283.

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female forms in the sub-species *polytrophus* and *tibullus* of *Papilio dardanus*. Plate II. of my last BEDROCK article would have shown this clearly to Professor Punnett if he had studied it even superficially ; for seven of the figures are distinctly labelled “ *Polytrophus* male ” and “ 6 *Polytrophus* females of 4 forms,” two of the latter being named “ *trimenti*.” No trouble is necessary in hunting up reference numbers in a description of the figures. All the information is printed on the face of the plate. I think I am entitled to use Professor Punnett’s words on p. 161 and ask for “ a more critical spirit.”

It is difficult to take seriously Professor Punnett’s reply to my criticism* of his statement that, according to the Darwinian view, a certain African Danaine butterfly arose direct but gradually from another, and that, according to the Mendelian, the origin was sudden.† I pointed out that no one had ever suggested such an origin at all, and that those who had studied the group placed the two species rather far apart. Professor Punnett’s reply is curious. The conclusions on the zoological affinities of his group reached by the great Swedish naturalist who has spent most of his life in the exact and careful study of African butterflies he likens to those of Moses, his own conclusions on the same question reached by no study at all, he compares to those of the modern zoologist ! This is the way in which he is concerned to defend an elementary exposition of his subject intended for the non-scientific public !

Returning for a moment to the female *Acraea alciope*, I think that the facts brought forward in the first number of BEDROCK, together with Mr. Neave’s more recent discoveries referred to in the present paper (pp. 302, 303), will convince the great majority of naturalists that the mimetic pattern was attained by steps and not suddenly. Yet the result is here far less elaborate than that seen in the two mimetic females of *Papilio polytes* which Professor Punnett maintains were produced at a single bound. I have never yet written on the evolution of these females, and, since the history as I interpret it is different from that which Professor Punnett ascribes, on pp. 153 and 154, to the followers of the theory of mimicry, it is appropriate that I should do so on the present occasion. I have

* BEDROCK, April, 1913, p. 52.

† *Mendelism*, 1911, pp. 134—5.

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the advantage of writing with several specimens from Ceylon as well as some from other parts of the geographical range beside me—the former collected and kindly given to me by Professor Punnett himself.

Professor Punnett's Plate II., facing p. 153, gives a fair idea of this butterfly and its two models. The reference numbers to the *hector* and *aristolochiæ* forms are unfortunately transposed on p. 153, and on the plate the intense red spots on the hind wings and the same vivid tint on the body of one model, *P. hector*, are invisible, while the dull red spots on the hind wings of the other model, *P. aristolochiæ*, can be detected with difficulty. The photograph required screens, special plates and long exposure to give a good reproduction of these difficult tints, but its failure has a special interest in relation to the present discussion. To the human eye the red of *P. hector* is so aggressively assertive that an æsthete of thirty or forty years ago would have declined to live in the same house with the butterfly; yet, upon Professor Punnett's photographic plate, it produced the same effect as black. The dull unobtrusive red of the corresponding mimetic female would have been tolerated or even welcomed at the period of "Patience"; yet it asserts itself on the plate and comes out in its true value against the black background of the wing. It is evident that the pigments are quite different, and spring from different genetic factors in model and mimic.

Professor Punnett supposes (p. 155) that the two mimetic females arose suddenly in their present form from the male-like female. "After all," he says, "the different females of *polytes* are doing the same sort of thing every day." Men with potential aptitude—more or less—for reasoning are born every day. Does Professor Punnett therefore believe that man as he is now arose suddenly from a common ancestor with the anthropoids? If not, the fact that the different females of *polytes* are produced now is hardly an argument that they were originally produced in their present form. I would ask any thinking naturalist to look at Professor Punnett's Plate II. and compare the two mimetic females (3 and 4) with their two models (5 and 6) and with their non-mimetic ancestor (2), to note carefully the various points of resemblance to the models and of difference from the ancestor—points analysed on pp. 308—10, and

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then consider whether it is reasonable to suggest that all these features in the pattern—some of them detailed and nearly exact, such as the V-like white marks near the apex of the fore wing in the "*hector* form" (3)—that all these arose suddenly and together in each of the two mimics. That a large variation may arise suddenly no one ever doubted, but not many naturalists will accept the view that a complex pattern of many elements resembling the corresponding elements in an entirely different species could spring into existence as a whole and complete in all its details.

Granting the sudden origin of the two mimetic forms, Professor Punnett admits, on p. 155, that they would be preserved and rendered predominant by Natural Selection, but it is difficult to reconcile this part of his paper with pages 156 to 158, in which he reaches the conclusion that the proportion of the mimetic females in Ceylon expresses a Mendelian equilibrium undisturbed by selection. *Papilio polytes* has a wide range in the Oriental Region. Over most of this range it is accompanied by one model only, and not two; in certain localities this single model is so scarce that it is impossible to believe that it can act as a model at all. With these facts, which enable him to ascertain what actually happens in the absence of one or both models (as effective agents), Professor Punnett light-heartedly chooses Ceylon, where both models are common, as his crucial locality, and, entirely neglecting comparison with other areas, concludes, with all the emphasis of italics, that "*Natural Selection is non-existent in so far as concerns the relation of the mimetic to the non-mimetic females of Papilio polytes*" (p. 158).

Now let us see where the facts lead. *Papilio hector* is only found over a part of the area of *polytes*, and of the second model *aristolochiæ*. Localities in this part, Ceylon being one, yield both forms of mimetic female. Localities in the area outside this part yield one mimetic female, the "*aristolochiæ* form." The species forms an interbreeding community, at any rate over the continental part of its range, and it is to be expected that a certain small proportion of "*hector* females" will stray into the country outside the boundary of their model. I only suggest this probability from an experience of *dardanus* and other mimetic species in Africa. In the Hong Kong and Macao districts *P. hector* is unknown, and so is its mimic: *P. aristolochiæ* is excessively rare, so much so that some

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observant naturalists long resident in these localities have never seen it at all. *Papilio polytes* is, according to these naturalists, the commonest or one of the two commonest swallow-tails of the locality. Here then is a splendid opportunity for Mendelian equilibrium. What we really find is the almost complete preponderance of the male-like female. I have lately received six females—all of this form—captured by Captain R. A. Craig on Stonecutter's Island, in Hong Kong Harbour. Not a single model was present in the collection. Dr. Seitz, in a long experience of the Kowloon district, never saw any other form of female and never saw the model. Commander J. J. Walker alone thought that the "*aristolochiæ* form" was as common as the non-mimetic form at Hong Kong, although he, too, never saw the model. Mr. J. C. Kershaw's experience at Macao corresponds with that of most observers at Hong Kong. He finds the male-like female of *polytes* is the common one and has never seen *P. aristolochiæ*.* The opposite condition is found in New Guinea, where the representative of *polytes* has but a single female form mimetic of the representative of *P. aristolochiæ* and the male-like form is unknown.

In order to reach safe conclusions we really need many cabinets filled with specimens of this butterfly and its models from localities scattered over representative parts of the range. Many thousands of specimens are required. But we already know enough to feel confident that the two mimicking females only occur regularly where the two models are common, and that when one model is absent and the other common, the corresponding mimetic female is common; finally that when the single model is wanting or extremely rare the corresponding form is absent or rare. The facts do not warrant Professor Punnett's italicised conclusion quoted on p. 306.

I will now attempt to trace the evolution of the two mimetic females of *polytes*. I do not believe for a moment that the species is palatable to insect-eating animals in general. The under-surface pattern of the male closely resembles its upper surface, only differing in that it is rendered even more conspicuous by the larger yellow marginal markings on the hind wing as well as by a row of red spots lying within these markings. Such a relationship between the

* See *Proc. Ent. Soc. Lond.*, 1913, pp. xxxi., xxxii., where observations in the Hong Kong and Macao districts are recorded and references given.

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upper and under surface is also found in *P. aristolochiæ*, and is very characteristic of the groups which supply the best-known models for mimicry. It is the very opposite of the relationship seen in butterflies with a dead-leaf-like or otherwise procryptically-coloured under-surface to the wings. The mimicry is, I hold, Müllerian, and the mimetic forms have merely exchanged the warning patterns peculiar to their kind for those characteristic of two other far more distasteful species with a more flaunting and slower flight. An exchange like this of one conspicuous pattern for another, when it can be established, seems to me a good criterion of Müllerian mimicry. We should expect a Batesian mimic to be developed out of a species with a procryptic under-surface. Professor Punnett accepts without question Haase's hypothesis that the distasteful qualities of the models are derived from the food-plant. Haase may be right, although I have always felt that stronger proof is required, but under any circumstances distasteful qualities can be elaborated in the body and are not necessarily borrowed direct or with slight change from the food-plant. Commander Walker has told me of the larva of the Australian *Papilio macleayanus*, feeding on the "Sassafras tree," *Atherosperma moschatum*, which emits from the well-known pair of glands behind the head a "strong and very disagreeable scent"—which is "totally unlike the pleasant nutmeg-like fragrance of the Sassafras, but resembles that of butyric acid or the smell of the little malodorous ants of the genus *Cremastogaster*."* Commander Walker even found that the caterpillars were more easily collected by smelling for them than by looking for them. A citronaceous food-plant is not evidence of the palatability to insect-eaters of *P. polytes*.

We now come to the transformation of the non-mimetic into the mimetic forms. Professor Punnett assumes (pp. 153, 154) that "on the theory of mimicry" the two forms were evolved independently; but I do not think there is the slightest doubt that the mimic with a far wider range, the "*aristolochiæ* form," was evolved first and that later on the "*hector* form" was developed from it and not direct from the male-like female. The essential and first change, upon which the detailed likeness to *P. aristolochiæ* has been built up, was, I do not doubt, the shortening and widening of

* *Ent. Mo. Mag.*, xli. (1905), p. 220.

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the yellow bar crossing the hind wing of the female *polytes*. The bar is not only widened by the lengthening of its central constituent spots, but by the appearance of a patch of yellow at the end of the cell. A very similar shortening and widening of a yellow bar crossing the hind wing is to be seen in an African Nymphaline butterfly, *Neptis woodwardi*.^{*} The mimetic transformation is here evidently very recent, and distinct progress is seen[†] when we pass from the N.E. shores of the Victoria Nyanza to the Kikuyu country, east of the Rift Valley, where the Danaine model (*Amauris albi-maculata*) is especially predominant, and other mimics of its pattern abound.[‡] Such a change in its most conspicuous element would by itself cause the pattern of *polytes*, upon the wing or at a little distance when at rest, to suggest that of *aristolochiæ*. The remaining features of the resemblance were then gradually added, each contributing something to the effect and suggesting more and more strongly the pattern of *aristolochiæ*: (1) the disappearance of the marginal yellow spots of the fore wing, (2) the emphasis and reproduction, on the upper surface, of the sub-marginal spots of the hind-wing under surface—already present in the male and generally red, although sometimes yellow, already tending to appear on the upper surface of some male-like females; (3) the peculiar light-and-dark striation of the outer half of the fore wing, and its reproduction with a marked brightening of the pale elements on the under surface; (4) lastly, the almost entire disappearance from the hind-wing upper surface of the yellow lunules marking the bay-like indentations of the margin—a character already extremely variable in the male and male-like female. I do not mean to imply that these changes took place in the above order or that none of them occurred simultaneously. Comparison with the "*hector* form" renders it probable that (4) was the last change (see p. 310).

Now that the elements in the resemblance to *P. aristolochiæ* have been analysed, the improbability of their all appearing together at the same moment is emphasised. That Mendelian heredity has probably played an important part at some of the stages I freely admit. Why Professor Punnett should prefer to think that

^{*} *Trans. Ent. Soc. Lond.*, 1908, p. 512.

[†] *Ibid.*, Pl. XXIX., Figs. 2 and 4.

[‡] *Ibid.*, Pl. XXVIII.

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the Mendelian principle can only act once in the history of a mimetic form I am at a loss to imagine. Some mimetic transformations are simple, some are excessively complex; he seeks to explain them all by a single variational leap. What reason has he for thinking that variational activity—whatever may be its unknown cause—like certain flowers, can only bloom once? Why should he seek to lay this hard burden on the Mendelian principle as a factor in evolution? This question is raised at the present point by the remarkable contrast between the simple evolution of the “*hector* form” and the complex transformation I have attempted to describe in the preceding pages. As regards the hind wing, the “*hector* form” possesses almost precisely the pattern of the “*aristolochiæ* form” with its yellow transformed into red—a change which may well have occurred suddenly. The comparison between Figs. 3 and 4 on Professor Punnett’s Plate II. shows the nature of the transformation, but to appreciate it fully the actual specimens should be studied. The marginal and sub-marginal lunular markings are larger in the “*hector* form,” and this is the only constant difference between the hind-wing patterns of the two forms. As regards the marginal lunules it is probable that the “*hector* form” arose before these markings had become evanescent, as they now are on the upper surface of the “*aristolochiæ* form.” These markings—although out of place in a mimic of *P. hector*—are red like the rest of the hind-wing pattern in the “*hector* form,” a probable indication that the change to red was a single transformation, involving some divergence from the new model, although, upon the whole, resemblance to it. At the same time the likeness is a rough one, for, as I have said, the hind-wing pattern, apart from its colour, is that of the “*aristolochiæ* form” and its model. The fore-wing pattern is doubtless the most conspicuous part of the “*hector* form,” and here the likeness to the model is far more convincing. It has obviously been produced from the striated fore wing of the “*aristolochiæ* form” by reducing certain parts and emphasising others, on both surfaces, and, upon the under, distinct traces of the increased paleness of the older form are retained on parts of the wing that are black in the model, *P. hector*.

How can we account for the evolution of two mimetic forms in a butterfly which remains dominant when its models are absent or

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excessively rare ? It is worth while to consider this question in some little detail, for I believe that the true explanation is different from that usually given.

Papilio polytes is an unusually dominant and successful swallow-tail. Its rate of reproduction, combined with a probable measure of distastefulness advertised by a conspicuous pattern, its powers of flight, alertness, and other adaptations of many kinds, keep up the large average numbers in spite of the attacks of enemies of all sorts in all the stages of its life-history. The large numbers that survive in every generation will, of course, include the fittest, and so the high level of protective efficiency is maintained. This is the condition of *polytes* in the Hong Kong and Macao districts where the single model is so rare that it is unreasonable to suppose that it exerts any effect, and this was doubtless its condition before the evolution of the mimetic forms. There is no reason to suppose that the surviving percentage of *polytes* was increased by the presence of the *aristolochiæ* model or during the growth of the mimetic likeness. All that happened was this : certain variations formerly unselected, now tend to fall into the surviving percentage, and, once started, the further stages of transformation were effected in the same way. Each change that suggested still more strongly an advertisement common to a far more distasteful form would tend to be selected. So, too, when *polytes* spreads beyond the range of *aristolochiæ*, or when the model for some reason disappears from an area in which *polytes* is abundant, the constitution, not the amount, of the surviving percentage is changed. The mimetic pattern soon disappears, although the species that bore it remains as abundant as before. The survival or extinction of the species is not affected : all that has happened is the survival or extinction of a pattern borne by a certain proportion of the individuals of the species. When these disappear other individuals with another pattern take their place. It is, furthermore, extremely probable that selection is reversed when the models are absent, for a female that resembles the male is better advertised than one which resembles a non-existent model. Although I believe that many mimicking species bear the above-described relationship to their models, I do not mean to imply that this is always so. No doubt there are plenty of mimicking species which depend upon the presence of the model for their

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existence and could not live in areas from which the model disappeared.

I have answered the main points raised by Professor Punnett, and should have been glad of the opportunity of discussing them all, but it seemed better to devote a considerable part of the present article to *Papilio polytes*, and thus offer what a Darwinian really believes as a substitute for what he is assumed to believe.

MATERIALISM, SCIENTIFIC AND PHILOSOPHIC

By William McDougall

IN his article on "Scientific Materialism" in the last number of *BEDROCK*, and in several earlier publications, Mr. H. S. Elliot appears as the exponent of an intellectual attitude which, I believe, is common to a large number of men of science at the present day. This attitude seems to imply some confusion of thought, the nature of which I will attempt to indicate very briefly, afterwards adding a few words on the more special question at issue between Mr. Elliot and myself.

Materialism is that way of thinking which regards all phenomena as strictly obeying the laws formulated by the sciences of inorganic nature. But there are two ways of holding this materialistic view of things, namely, the scientific and the philosophic. Philosophic or Metaphysical Materialism accepts as literally true the description of the world given by the physical sciences; and, taking this assumption as a premise, logically deduces a negative answer to a number of questions of the highest importance, questions to which the spiritualist philosophy returns a positive answer, or which it at least regards as open.

Scientific Materialism also accepts the view that the principles of physical science are valid of the whole world of phenomena presented to the senses; but it accepts this view as a working hypothesis or guiding assumption only, and does not hold itself justified in deducing from it a confident negative to questions which lie outside that province the study of which has led to the enunciation of those principles, the province, namely, of inorganic nature.

These two kinds of Materialism were not differentiated in the thought of the ancients, partly because the distinction between science and philosophy had not been drawn, and partly because

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the ancient materialists, and indeed, the thinkers of antiquity in general, did not clearly distinguish between matter and spirit or between physical and psychical processes. Mind, soul, or spirit was for most of them but the subtlest kind of matter, a fluid composed of the finest and most mobile particles ; and matter partook of some of the attributes of mind. Centuries after Plato had laid the foundation of the modern distinction between mind and matter, Lucretius, the most systematic exponent of atomic Materialism, still ascribed some power of voluntary movement to the atoms ; and many of the Stoics and of the Christian Fathers, who were materialists without being atomists, regarded the soul as a subtle material vapour. It was not, indeed, until Descartes had defined matter as extended and mind as inextended substance, that the distinction between matter and mind, or between physical and psychical processes which is now commonly recognised became established in the philosophical tradition. From that time onward philosophers have been very largely occupied with the task of achieving a satisfactory statement of the relation between the physical and the psychical ; and, according to the answer they return to this question, they must rank (agnostics excepted) as either spiritualists or philosophic materialists.

But it is by no means easy to avoid confusion when we attempt to arrange modern thinkers under these two heads. Only the dualists on the one hand and the literal materialists on the other can be assigned to these two classes respectively without careful enquiry. Many modern philosophers, as well as, probably, the great majority of men of science, meet the difficulty of stating the relation of mind to matter by accepting the principle of Psycho-physical Parallelism. Now, the confusion between Scientific and Philosophic Materialism is largely due to the fact that the name Psycho-physical Parallelism is claimed for a number of widely different doctrines which have one feature only in common, namely, the denial of interaction between psychical processes and the physical processes of the brain which are generally believed to accompany them in some regular and lawful fashion. In spite of having this point in common, these doctrines range from Epiphenomenalism on the one hand (which regards all psychical processes as wholly dependent upon the processes of nervous

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systems, and as without influence of any kind upon the course of events) to *Psychical Monism* on the other, the doctrine that psychical processes alone are real, that all real process is of the nature of the psychical activities of which each of us is immediately aware, and that all physical phenomena are merely the appearance to us of such psychical activities, somehow assuming for us this peculiar disguise.

Psychical Monism necessarily claims to be metaphysically true; it is *Spiritualism* of the most pronounced kind. But *Epiphenomenalism* either may be held as a convenient and useful working hypothesis; when it justifies *Scientific Materialism* only. Or it may be regarded as metaphysically true; in which case it implies all the more important consequences of literal *Materialism*: and those who accept it in this sense may properly be classed with the philosophic materialists. A similar, or rather a more perplexing, ambiguity characterises many other statements of *Psycho-physical Parallelism*; for not only is it in many cases difficult to gather whether they are put forward as working hypotheses or as metaphysical theories; but also, it is not always easy to determine in these latter cases whether or how far they necessarily imply the same consequences as *Philosophic Materialism*.

Now, many men of science, especially perhaps among the biologists, appreciating in some degree the objections to literal *Materialism* that have brought it into disfavour among philosophers, give a general adhesion to the principle of *Psycho-physical Parallelism*, generally without attempting to make clear which form of that principle they mean to accept. And in so doing they feel that they take up a philosophic position rendered respectable by the approval of all those eminent philosophers who have accepted *Psycho-physical Parallelism* in one form or another. They are thus strengthened in, or encouraged to yield to, that tendency to adopt the conclusions of *Philosophic Materialism* which devotion to the natural sciences almost inevitably fosters; and, towards a number of questions of the highest importance, they become confirmed in an attitude of dogmatic negation, one which is justified only by *Philosophic Materialism*, but is not necessarily implied by or justified by the principle of *Psycho-physical Parallelism* as held by its most authoritative exponents. That is to say, the ambiguity involved in so many statements of

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this principle leads them to believe that by declaring their adhesion to it they place themselves in the best philosophical company ; yet this belief is a delusion, for, while they interpret the principle in the sense of Philosophic Materialism, most of the philosophers in whose company they believe themselves to be hold the principle in a way which justifies Scientific Materialism only.

The fact may be illustrated by reference to the views held by the chief exponents of Parallelism on some of those problems a dogmatic negative to which is falsely held by the scientists in question to be justified by the principle. These questions are important because they have direct bearing on those most important of all questions—What am I ? What may I hope for ? What ought I to do ? They mostly fall into three groups : (1) the moral questions, such as “the existence of God,” “the freedom of the will,” and “the real efficiency of volition” ; (2) all those biological questions an open mind toward which is the mark of the modern vitalist ; (3) the questions with which “psychical research” is chiefly concerned, such as the possibility that the death of the body does not necessarily involve the entire destruction of the person, and the possibility of the direct communication of mind with mind.

Spinoza and Leibnitz were the originators of “Parallelism” ; yet the former, although sometimes wrongly referred to as an atheist and materialist, has also been described as “the God intoxicated man” ; and the latter affirmed the existence of God and regarded the human soul as a being created by Him and not dependent upon the life of the body. Kant, who at least inclined to accept “Parallelism,” if he did not definitely adopt it, was the greater defender of God, freedom, and immortality, against the literal Materialism of the eighteenth century. Fechner, to whose influence the present day popularity of “Parallelism” is chiefly due, argued at length again and again throughout his long career for the reality of the life after death. Paulsen, the chief exponent and populariser of Fechner’s philosophy, and a thorough-going parallelist, claimed that the doctrine was compatible with belief in human immortality. The leading contemporary exponents of “Parallelism” are Professors Wundt and Heymans. Neither of them interprets the principle in the sense of Philosophic

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Materialism. Wundt regards volitional or conative effort as more real or actual than any of the processes described by physical science ; and, applying this conception to biology, regards all the innate tendencies of men and animals as expressions of organisation built up by the conscious efforts of foregoing generations ; he thus accepts the Lamarckian principle of the transmission of acquired characters, the dogmatic denial of which is one of the biological consequences of the interpretation of "Parallelism" in the sense of Philosophic Materialism. Lastly, Professor Heymans has recently contended ("In Sachen des psychischen Monismus," *Zeitsch. f. Psychologie*, Vol. 64), that many of the alleged facts, the critical examination of the evidence for which is the peculiar task of "psychical research" (especially those implying telepathy and the survival of personality after the death of the body), are not only reconcilable with Parallelism, but are just what may be reasonably expected by the parallelist.

Whether these views are in all cases logically justifiable is a fair question ; but the citation of these instances will serve to show that Parallelism is held by its leading exponents to justify only Scientific and not Philosophic Materialism ; and that, therefore, men of science are mistaken when they believe that their acceptance of the principle of Psycho-physical Parallelism gives philosophic justification and respectability to their dogmatic negatives in the spheres of morals, of psychology, and of biology.

The confusion between Scientific and Philosophic or Meta-physical Materialism, which, as we have seen, is so largely due to the ambiguity of the notion of Psycho-physical Parallelism, is the distinctive feature of what Mr. Elliot calls "Materialism, in the new sense," that Materialism which he champions and which is so widely current among biologists at the present time. It is put forward as identical with the Scientific Materialism to the elaboration of which, by the great physicists and mathematicians, the triumphal progress of physical science has been so largely due. But Mr. Elliot, in common with many biologists, interprets it in the sense of Philosophic Materialism. On the other hand, the physicists who have built up Scientific Materialism have, for the most part, remained conscious of its proper limits, and have not pretended to deduce from it any conclusions outside the sphere

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of physical science. Newton, Boyle, and Priestley were scientific materialists, but they believed nevertheless in a Divine Creator; and, though, since their time, almost all physicists have been scientific materialists, many of the greatest of them have explicitly repudiated Philosophic Materialism. In this connexion our great historian of nineteenth century thought may be cited.

“It is not,” says Dr. T. Merz, “pre-eminently among such natural philosophers as define and handle the fundamental principles of the mechanical view with the greatest accuracy and efficiency that we find the materialistic view of the world prominently put forward. It is rather by those thinkers—notably biologists—who are forced by training and habit to use such terms as mass, force, energy, cause and purpose in a wider and more pregnant sense than a purely mechanical definition would permit, that we find these conceptions employed to explain both mechanical and mental phenomena, and the claim put forward to establish a monistic creed. Mathematicians such as Gauss, Cauchy, Kelvin, Hertz, and others have always laid down their mechanical principles with the greatest caution, indicating or distinctly expressing the conviction that the phenomena of life and mind belong to an entirely different sphere of thought and research.” *

Mr. Elliot and other adherents of “Materialism in the new sense” (i.e., Philosophic Materialism masquerading in the guise of its scientific brother) dogmatically repudiate from the fold of science every kind of investigation or reasoning which refuses to be limited by their illegitimate deductions from Scientific Materialism, regarding them as manifestations of sentiment, superstition, or pure folly. The arbitrary definition of the nature of science implied by this procedure is again the product of the confusion between Scientific and Philosophic Materialism, and of the natural but ill-founded prejudice in favour of mechanical explanations. But, since Mr. Elliot will regard any reasoning of mine on this point as vitiated by sentimental prejudice, I will cite the mature opinion of a philosopher and man of science whose competence and impartiality are indisputable.

“That the vital processes,” writes Mr. L. T. Hobhouse in his latest work,† “must be ultimately of a mechanical character

* *A History of European Thought in the Nineteenth Century*, Vol. III., p. 584. London, 1912.

† *Development and Purpose*. London, 1913, p. 243.

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and that they are capable of scientific treatment, are in fact two quite different propositions; . . . The second proposition— . . . assumes, no doubt, that they can be clearly and adequately conceived, and it implies that so far as they are complex they can be resolved, by methods familiar to science, into simpler constituent factors. It does not, however, imply . . . that they contain no element which is unanalysable. On the contrary, it may always be one of the results of analysis to exhibit certain lowest terms as the final products of its work. All that is necessary for accurate knowledge is that these lowest terms should be definite elements clearly presented to the mind. As long as we can justly apprehend their nature, trace the combinations into which they enter and their behaviour therein, and record the difference which their presence makes in our world, they are subjects not merely of knowledge, but of the systematic and consecutive investigation which we call science. But, the objector may contend, these unanalysable data, if they are to be the subject of scientific treatment, must be of a mechanical character, and lend themselves to mathematical computation. This is in substance to identify science with mathematics. But for this identification there is no warrant in the postulates of thought. These postulates no doubt lay down that anything that exists must have its place in a system of relations which, when adequately defined, will be found to hold universally. But they say nothing whatever as to the character of those relations, and the conditions of universality and necessity do, in fact, attach as clearly to the means which serve an end, or the functions which together maintain an organic whole, as to the mechanical sequence of cause and effect."

In these words Mr. Hobhouse summarises the claim, of which his whole book is a vindication, that biology may refuse to limit itself to mechanical explanations without ceasing to be scientific.

Turning now to Mr. Elliot's article in the last number of *BEDROCK*, I confess myself at a loss what to say. I had written what I believed to be a convincing demonstration of the ineffectiveness of Mr. Elliot's criticisms of my *Body and Mind*; and he replies by flatly denying that I have met any one of them; Mr. Elliot says he regards most of what I write as the merest quibbling; and his frankness encourages me to make a similar candid confession in regard to his article. Our minds seem to be so hopelessly separated that they cannot come into effective contact; there seems to be no possibility of profitable discussion between us. But it is, perhaps, worth while to attempt to convince Mr. Elliot that, in respect of at least one matter in dispute between us, he has failed to appreciate my very

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obvious point ; for, if this can be accomplished, Mr. Elliot may be led to suspect that the rest of my arguments are not so pointless as he supposes them to be. I had said that, if a pencil (whether held in the hand of an automatic writer, or not so held) should write down a number of statements of facts which were known only to some deceased person (reproduced, let us say, with accurate detail the contents of a long letter written in secret, sealed, and locked away, immediately before the death of the author), such writing would afford strong reason for believing that the personality of the author had in some sense and manner survived the death of his body. In his criticisms of my *Body and Mind* Mr. Elliot sought to entertain his readers by citing parts of this passage in such a way as to give the impression that it claimed the mere occurrence of automatic writing as evidence of such survival. In my reply I protested against such treatment. But Mr. Elliot is either so obtuse or so disingenuous that he cannot or will not take my point ; and in his second article he persists in his misinterpretation of the passage, marvelling at length that a person of my antecedents should be so simple-minded as to regard the mere occurrence of automatic writing as evidence of a future life. He compels me, therefore, to say, in the baldest fashion, that I do not regard automatic writing (with which I am sufficiently familiar) as in itself affording any such presumption ; that the content or meaning of the passages written (the kind of knowledge revealed by them) could alone afford such evidence. Mr. Elliot must be aware that the efforts of the Society for Psychical Research have for some years past been largely concentrated on the critical examination from this point of view of the contents of automatically written passages. If he is not aware of this fact, he has no right to refer to investigations of this nature in the supercilious tone he adopts ; and, if he is aware of it, his error must surely be of the heart rather than of the head. But Mr. Elliot compels me to be very explicit ; and I must therefore say very plainly that he stands convicted in this matter of either extreme obtuseness or wilful misrepresentation. I will add that this one instance of his criticisms and of his dealings with my replies seems to me a very fair sample of the whole of them.

In conclusion, I would say a word about the use of the term "materialist." I am glad that Mr. Elliot accepts the designation ;

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for it may, I think, be properly used to denote not only the literal materialists, but also all those who interpret their "Parallelism" in the sense of Philosophic Materialism. It was not applied by me as an opprobrious epithet; for surely we have outlived the age in which "materialist" or "atheist" imputed immorality; yet Mr. Elliot seems to feel that some courage is needed for the acceptance of it. And he himself, on casting about for a term of opprobrium for the belief in God and the human soul, can find none better than "degrading type of materialism." In my reply I said that such misuse of terms was a matter to be dealt with by the Editor rather than by me. Mr. Elliot in his last article treats this as an appeal against freedom of speech; whereas, of course, it was a protest against a gross misuse of language, and the perpetuation of the bad old fashion of using "materialist" as a term of abuse pure and simple. I might equally justly describe Mr. Elliot's views on biology as a cranky type of spiritualism. Let Mr. Elliot describe those who do not share his views as superstitious fools, or use even stronger language, and I shall raise no objection. But this abusive use of "Materialism" is as much to be regretted as the eulogistic use of "idealist" so common among philosophers. Above all, I would have him and those for whom he speaks cease to believe that for their "Materialism in the new sense" they can claim the support of those great physicists who have built up Scientific Materialism, or of those philosophers whose solution of the psycho-physical problem is the principle of Parallelism; and I would have them realise that the position they so confidently maintain involves a confusion of Scientific with Philosophic Materialism.

THE TRANSMUTATION OF THE ELEMENTS

By Norman Campbell

THE alchemists were very severely practical people. Popular imagination is perhaps apt to picture them as dreamy recluses whose concerns were yet further removed from the affairs of the busy world than those of the most typical "scientific professor" of to-day. Really they had no care for pure learning whatsoever ; when they sought the Elixir of Life or the Philosopher's Stone their search was inspired by motives which are shared by the least intellectual stockbroker ; they wanted to live a long time and to be rich. And hence it comes about that the problems of alchemy retain a perennial interest for the reader of the modern newspaper, who also has an enthusiasm for "useful" science and a supreme contempt for any which is, according to his standards, "useless." A great advance in pure science may occasionally be noticed in a brief paragraph in the *Times* ; but anyone who claims to have discovered the Origin of Life or to have attained the Transmutation of the Elements can gain the satisfaction of seeing his achievement proclaimed in flaring headlines by the more popular press.

However intelligent people are beginning to realise that these startling discoveries are seldom heard of again after their first announcement. Serious men of science do not publish their researches in the newspapers and have no desire for the notoriety which they could obtain by doing so ; only those who have failed to secure or to retain the lasting respect of the experts are likely to desire the momentary plaudits of the ignorant. But it is not a necessary law of nature that every scientific statement in the popular press cannot be both new and true, and it would be unwise

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to carry too far a scepticism based on the many blunders to which the craving for sensationalism gives rise. When such a very circumstantial account appears as that which was published last February by the *Morning Post* and other papers, describing experiments and conclusions based on them which, if they were substantiated, would represent a very notable scientific achievement, it would be foolish to be completely incredulous merely because the manner of publication was unusual. It is probable that there are persons, interested in modern science but having no first-hand knowledge of recent advances in physics and chemistry, who desire to know what is the present position with regard to the Transmutation of Elements and how far it has been changed by the recent work of Sir William Ramsay and of Messrs. Collie and Patterson. The following pages are an attempt to provide such information.

An alchemist would not, of course, have spoken of the Transmutation of the Elements at all; he sought the Transmutation of the Metals and was interested only when the metals concerned were gold on the one hand, and, on the other, the "base metals," such as lead. The two problems are connected only because the metals which the alchemist sought to transmute happen to be included among the elements of modern chemistry. Our first task then will be to enquire what we mean by "elements" and why we should be peculiarly interested in their transmutation.

Experiment shows that by suitable means most of the substances known to us can be resolved into substances differing from them and can be reproduced by compounding these other substances. When we attempt to push further and further the process of resolution, substances are eventually reached which cannot be resolved by any of the means which were used to resolve the original substances. The nature of the irresolvable substances which are ultimately obtained by resolving a given original substance is, speaking generally, independent of the particular means of resolution adopted and their number is very small relatively to that of those which can be resolved. Observations of this nature, doubtless perfectly familiar to everyone, give rise to the theory that all the resolvable substances are composed of the irresolvable substances,

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which are termed elements, and lead to the definition of an element, given in some primers of chemistry, as a substance which cannot be resolved into anything different from itself and cannot be produced by compounding any substances different from itself.

If we were to adhere perfectly rigidly to this definition, we could hardly enquire whether a transmutation of the elements is possible. For, if a substance could be converted into anything different from itself (except possibly by a direct transformation in which nothing was added to or subtracted from it), it would cease to satisfy the definition and should no longer be called an element. We could not logically enquire whether a transmutation of the elements is possible, but only whether the substances which we now call elements are rightly so called. But men of science do not adhere strictly to definitions in this way (to the distress of some pure logicians) ; they define their terms so as to make convenient the description of the facts which they discover, and, if they subsequently find that the facts are not exactly what they had imagined, they are quite prepared to change their definition rather than abandon a convenient form of description. Even if we found that the substances which now we call elements could be resolved or compounded or transmuted, we should probably still continue to call them elements so long as one of two conditions were fulfilled. The first condition is that the means by which the resolution of the substances could be effected formed a distinct class, easily distinguishable from that of the means, mentioned above, by which it could not be effected ; the second condition is that it should still be found that the substances called elements possessed some important common quality besides that of being irresolvable. For if either of these conditions were fulfilled the substances which are now called elements would still form a distinct class very different from that of compounds and mixtures, and it would still be desirable to have a separate name to denote them. We should doubtless change the definition of the term "element," but not the extent of the class defined by it.

Now the second of these conditions certainly is fulfilled ; the substances which we call elements do possess common qualities besides that of irresolvability by certain methods. The elements may be divided into several well-marked groups, each member of a group resembling closely the other members of that group and

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differing completely both from other elements and from all compounds. If a list of the elements is made in the order of increasing atomic weight, the element with the lightest atom being put first, that with the next heaviest atom second and so on, it is found that the elements of a single group occur at regular intervals in the list. Thus the 3rd, 11th, 19th elements belong to one group ; the 4th, 12th, 20th to another ; the 5th, 13th, 21st to a third ; and so on. A table of the elements so arranged as to make clear this recurrence of the same group at periodical intervals is called the Periodic Table and the proposition stating these relationships is called the Periodic Law. If this law is true, every element must have a definite place in the table ; if there is a place in the table without an element to fill it, we suspect that there is yet another element to be discovered, and if there is an apparent element without a place in the table, we suspect that it will turn out not to be an element in other respects. Thus, if we found a new substance, apparently elementary, having an atomic weight between those of the 19th and 20th elements, but resembling the 5th element in properties, there would be no place for it in the table, and the claims of the substance to be an element would be regarded as very doubtful. Suppose on the other hand that, in place of the facts just described, we had found that the 3rd, 11th, 18th formed one group, the 4th, 19th another, the 5th, 12th, 20th a third, there would be strong reason for suspecting the existence of an undiscovered element between the 11th and 12th, belonging to the same group as the 4th. The relationships established by the Periodic Law are so marked and so well established that we might (and in fact probably do) use them to define what we mean by an element ; we might say that an element is a substance for which a consistent atomic weight can be measured so related to the properties of the substance that the substance has a definite place in the Periodic Table ; such a definition would avoid any difficulties which might arise if the Transmutation of the Elements were effected, and would not prejudice in any way the question of whether such transmutation is possible.

Now it is the relationships stated by the Periodic Law which render so interesting the question of the Transmutation of the Elements. Science consists not in the discovery or contemplation of facts, but in the explanation of those facts, the discovery of some

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principle of which the facts are a necessary consequence. As soon as it was discovered that any connection could be traced between the various elements, speculations began to be advanced that the elements must possess some common constituent in their structure ; a theory based on the existence of such a constituent might obviously explain the connection. The theories hitherto advanced have not been very successful in attaining an adequate explanation ; even since we have had quite definite information of a constituent common to all elements little progress has been made in solving the mystery of the Periodic Law. But as soon as we imagine that the atoms of different elements are built up of similar parts differently arranged, the possibility of changing the atom of one element into that of another is brought forcibly to our minds. It is still possible that one part of the structure of the elements is common to all while another part is peculiar to each element and that therefore transmutation is impossible, but such an hypothesis should not be entertained while the simpler and more attractive alternative has not been finally disproved. The Periodic Table suggests the possibility of transmutation, and the information which we should obtain if we could effect transmutation would doubtless go far towards enabling us to explain its relationships.

Probably much more research in this direction has been done than the world has ever heard of. Investigators are sometimes a little ashamed of their bolder speculations and disinclined to publish a record of their heroic failures. But it is certain that at the end of the last century, when the Periodic Law was fifty years old and simpler relationships of the same kind had been known yet longer, no evidence whatever had been produced that any change in the nature of an element could be produced. The opening years of the new century brought a revolution. It is now one of the best established facts of science that one element can change into another ; whether it can *be* changed is not so certain.

The evidence that has produced so great a change in our knowledge is derived from the new science of radioactivity. The fundamental facts of this science and the chief arguments that can be based on them have been described so often for the benefit of the lay reader that an acquaintance with them might almost be assumed. But perhaps it will be well to state them once more as concisely as

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possible in such a way as to bring into prominence the various steps in the reasoning which underlies the belief in the occurrence of a Transmutation of the Elements.

There are certain bodies, called radioactive, which emit continuously certain rays which produce various effects which can be readily measured. Among these bodies are those which contain some of the elements which have been long known to chemistry. If a certain amount of such a body, A, originally chemically homogeneous, is left to itself for a space of time and then examined, it is found that the substance is no longer chemically homogeneous; there can be separated from it by chemical methods a substance B, which is also radioactive, but differs from A both in its chemical properties and in the nature of the rays which it emits. The amount of B which can be thus separated from A is so small that its nature cannot be investigated by the ordinary methods of chemistry. The theory of this phenomenon, advanced by Professors Rutherford and Soddy, asserts that the substance which is contained in B and confers on B its radioactive properties is an element different from any contained in A and produced by a change or transmutation of one of the elements contained in A.

In order to establish this proposition two things must be proved : (1) that B contains an element which is not contained in A, and (2) that the new element contained in B is produced from one contained in A and from that alone ; the hypothesis that it is produced by the combination of two or more elements contained in A must be excluded. It will be convenient to consider (2) first.

Convincing and very direct evidence that the substance, whatever it is, which is responsible for the radioactivity of B is derived from one of the elements in A and from that only is afforded by the fact that the rate at which B is produced from A depends only on the amount of one of the elements in A which is present in that substance and on that only. If a certain amount of this element, a , is present in A in given amount, then the rate at which B is produced from it is the same however the composition or circumstances of A may vary in other ways. A more complete proof of the proposition (2) is inconceivable.

But there is yet other evidence. If B is produced from a , a should diminish in amount as B is produced. Now when a is one of the

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elements previously known no diminution in its amount has ever been detected. But if B is radioactive, then it is always found that, if B is left to itself, there is again produced in it a third substance C, differing from B as B differed from A, and that as C is produced the radioactive principle in B diminishes. It is possible therefore that we fail to note a diminution in α as B is produced only because that diminution is so slow that it cannot be detected in the time that we have been able to investigate it. And this supposition is rendered very plausible by another fact. An empirical relation has been found between the nature of the rays emitted by B and the rate at which it disappears as it produces C ; if we suppose that this relation holds also between the rate at which α disappears and the nature of the rays which it emits, then that nature is found to be such that the rate of its disappearance would be so small as to be quite impossible to detect in a human life-time.

Now for (1). The substance responsible for the radioactivity of B is certainly not the element responsible for that of A, for the nature of the rays emitted by B is different from that of those emitted from A. Therefore, if this substance is an element, it is an element different from α . Two very direct reasons for believing that the substance responsible for the radioactivity of B is an element may be given. In the first place, the substance responsible for the radioactivity of A is certainly an element, namely the element α ; for the rate at which A emits rays, like the rate at which it produces B, is proportional to the amount of α present and depends on nothing else. If then the radioactive principle of A is always an element, there is a presumption that the radioactive principle of B is also an element. In the second place, if B is subjected to further chemical treatment, it is found that its radioactive principle behaves in a manner closely similar to that in which the elements of one of the recognised chemical groups behave ; that is to say, so long as only those chemical methods are employed which would separate from B all the elements of every group but one, the activity of B is unaltered, but if a method is employed which would separate from B the elements of this group, then the activity is separated from B and transferred to the material separated from it. But if the chemical properties of the radioactive principle of B enable it to be placed definitely in one of the groups of elements, it is probable that

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it is an element. In the third place the change in a which produces B is certainly not in any way similar to any other change which we know may take place in an element ; for no other change is known the rate of which is wholly unaltered by any treatment to which the element may be subjected, and none in which the energy liberated when a given amount of a is changed is nearly so great. Accordingly the change from a to B is not a chemical combination of a with itself or any other element or a modification of the physical state of a ; it is some change of which we had no experience before the discovery of radioactivity. The only change in an element which is clearly conceivable but had never been observed previously is a change of that element into another.

These are the most direct, but by no means the most convincing proofs of the theory of Professors Rutherford and Soddy ; the others are based primarily on an hypothesis as to the nature of the change in a which produces b , the elements responsible for the radioactivity of B . The element b may be produced by a change in the element a in two ways ; either two or more atoms of a may combine to form an atom of b , or the atom of a may break into two or more parts of which one is an atom of b ; all evidence points to the second alternative. For when the nature of the rays emitted by any radioactive substance is examined it is found that they consist of three kinds, of which two are formed by particles travelling with high speeds. The mass of these particles can be measured ; the particles forming one kind of rays (the so-called β rays) have a mass very small compared with that of any atom ; those forming the other kind of rays (α rays) have a mass comparable with that of the lighter atoms ; the earlier experiments of Professor Rutherford indicated that these particles might be atoms of the element helium, and this suggestion has been confirmed by evidence from various sources, of which one is an experiment (repeated since by other observers) in which Sir William Ramsay and Professor Soddy detected an apparent production of helium from a radioactive substance.* Whence do these particles come ? Professor Rutherford

* An extraordinary amount of prominence has been given to this experiment in the lay press, and the extravagant assertion has been made that it provided the first proof of the possibility of the Transmutation of the Elements. Historically this assertion is inaccurate ; the experiment was not made until the theory of

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suggested that the change which the atoms of *a* undergo when they are radioactive is a breaking-up or disintegration, that an atom of *b* represents one of the parts into which the atom of *a* splits, and an α or β particle another. There are many lines of argument which confirm this idea, and show that, in almost all cases, the atom of *a*, when it disintegrates and becomes an atom of *b*, produces at the same time only one particle, forming, according to its nature, an α or β ray.

Accepting this conclusion we can calculate from the known atomic weight of the element *a*, those of the elements which are produced successively from it ; for if *a* in producing *b* produces at the same time an α ray, the atomic weight of *b* must be that of *a* less that of a helium atom ; if it produces at the same time a β ray, the atomic weight of *b* is practically the same as that of *a*. Now when we know the atomic weights of such elements as *b* and *c* we can investigate whether those weights are so related to the chemical properties of those elements as to give them a place in the Periodic Table. This line of enquiry has been pursued recently and it has been shown that each of the newly discovered radioactive elements has indeed its proper place in that table† ; thus at the same time

Professors Rutherford and Soddy was generally accepted and it was believed by all those conversant with the subject that the Transmutation of the Elements was proceeding in every radioactive body. Logically the assertion is ridiculous. The experiment did not prove that helium was produced from some element other than helium, for (1) helium was produced in the experiment from a substance which was not known on chemical grounds to be an element ; it was produced from radium emanation. Now, if radium emanation was known to be an element, the Transmutation of the Elements had been observed already, for the production of that substance from a known element had been observed ; if it was not known to be an element the production of helium from it proved nothing about the Transmutation of the Elements. (2) No evidence could be derived from the experiments for the necessary proposition (2) of p. 327 ; owing to manipulative difficulties the helium produced was not found to be proportional to the amount of the substance from which it was supposed to be produced. The experiment was a triumph of skill, but scientifically it represented nothing more than a striking, but not very convincing, confirmation of the Rutherford-Soddy theory by which its result had been predicted.

† Perhaps this statement is rather bald. In order to fit all the new elements into the table certain modifications of our ideas of the Periodic Law have to be made, but I think it would be generally agreed that these modifications, while making the Law more complicated, make it also more comprehensible and show the possibility of removing some of the discrepancies in it which had previously been noted.

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are confirmed the conclusion that b and c are indeed elements (for our real definition of an element is that it should have a place in the table) and the hypothesis that in the production of B from A there is concerned a true transmutation or disintegration of the element a .

Probably the reader has found this very condensed account of the modern theory of radioactivity intolerably dull. Anyone who reads this article will have probably read it all before. I have gone through it all because it is so important to realise what is the nature of the evidence on which the occurrence of the Transmutation of Elements has been accepted hitherto, if we are to discuss evidence for its occurrence in other conditions.

Before we pass on two very important questions must be noticed. First, do the facts that have been described render it probable that the transmutation of elements other than those now recognised as radioactive occurs? This is a very difficult question. There is certainly nothing about the radioactivity of the known radioactive elements which indicates that there are no others; it is quite probable that there are other elements of which the activity is so slight that we have not been able to detect it. For if there are elements differing from the least active radioactive elements as much as these differ from the most active, no experimental means we possess at present could show that they are not devoid of radioactivity. On the other hand there is one argument against any of the other known elements being radioactive; all the known radioactive elements have very great atomic weights and, though the activity of these elements is related in no simple way to their atomic weights, there is some reason in this fact for suspecting that the remaining elements, which are all of smaller atomic weight. The question must be left completely open.

Second, how do these facts affect the probability that the transmutation of the elements may be effected artificially? Personally, I should say that they render it highly improbable that artificial transmutation is possible. It cannot be insisted too often that the radioactivity of the radioactive elements is independent of all external conditions; whatever we do to these elements we cannot vary in the smallest degree the rate at which they are changing. Now if, in those cases when we know transmutation is possible,

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nothing that we can do can accelerate or retard the rate of transmutation, surely it is improbable that, in those cases in which we have no reason to believe that transmutation is possible, anything that we can do can cause transmutation to occur. Such is the conclusion that I should draw and I believe that it is drawn by most men of science.

But there is one notable opponent of this opinion. For the last six years or more Sir William Ramsay with various collaborators has sought to achieve artificially changes from one element to another similar to those which take place spontaneously in the process of radioactivity. Nobody could be more fully equipped as an experimenter to undertake such work, for he had at his command the wonderful technique for handling and investigating minute quantities of matter which he had developed in the course of his researches on the gases of the atmosphere ; but perhaps in attacking a problem which offers so many opportunities for diverse interpretations of the observations, a healthy scepticism and a faculty for determined criticism would be almost more valuable than the most finished experimental skill.

The method by which it has been sought to produce transmutation has been practically the same in all cases. Some non-radioactive form of matter is subjected to the action either of the α or β rays from a radioactive substance or of other rays closely similar in their nature. Now if it were possible to produce transmutation, the action of such rays would certainly be the most likely agent known to us to produce it ; for such rays are known to penetrate through atoms and to act upon their internal and most characteristic structure in a way that it is probable that no other agent can do. But on the other hand careful experiments have proved conclusively that the disintegration of the radioactive elements is not influenced in any way by the rays which they emit ; it proceeds at the same rate whether the atoms are widely separated, so that the rays from one do not fall on another, or whether they are closely packed ; as has been said already, in the opinion of many people this fact renders it unlikely that the rays could produce any disintegration in an element in which it was not occurring spontaneously.

In conjunction with Mr. Cooke, Sir William Ramsay first attempted to show that under the prolonged action of such rays a substance

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originally not radioactive could acquire the power of radioactivity. Since, so far as we know, radioactivity is always associated with disintegration of the atoms, this observation would have proved that the substance had been caused to undergo such disintegration. But other workers have failed to confirm the observations, the apparent result of which was undoubtedly due to a contamination of the substance with true radioactive matter which it is very hard to avoid in laboratories where large quantities of such matter are used.

Subsequently with Mr. Cameron he investigated the action of the rays* upon water and various solutions. The Curies and other observers had shown that the α rays were capable of decomposing water into its elements, oxygen and hydrogen ; since, as has been said, the α rays consist of atoms of helium, a small proportion of that element is, of course, always found among the gases evolved. Sir William Ramsay and Mr. Cameron investigated the matter more nearly and found among the gases evolved, not only helium, but also the closely allied element neon. They found also that there was less oxygen among the gases evolved than would correspond to the amount of hydrogen. They advanced the hypothesis that some of the helium had been caused to unite with the oxygen to form the neon ; for the atomic weight of neon happens to be the sum of those of helium and oxygen. Unfortunately neon occurs in the atmosphere and it did not appear that these observers had taken sufficient care to exclude the introduction of neon from that source. Professor Rutherford and Mr. Royds repeated the work, showing that no neon appeared in the evolved gases if care were taken to exclude all air, and that the amount of air present in the earlier work was quite sufficient to account for the amount of neon found. The deficiency of oxygen has been traced with almost complete certainty to the formation of another compound of oxygen and hydrogen.

Next the same observers announced that, if the rays acted on a solution of copper salts, traces of lithium were formed, whereas if no copper was present no lithium was formed ; they supposed that

* In most of these experiments the radioactive substance was placed in actual contact with the liquid, so that the possibility of some action other than that of the rays was not excluded.

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under the action of the rays the element copper had been transmuted or disintegrated with the formation of the element lithium. The work was repeated by Mme. Curie and Mlle. Gleditsch, who showed that the lithium might have been derived from the glass vessels which were used ; the element is ubiquitous and is contained in almost all materials except the most carefully purified platinum. They could detect no difference in the amount of lithium produced whether or not copper were present. No attempt has been made to justify the earlier conclusion in the face of this criticism.

Lastly, Sir William Ramsay and Mr. Gray announced that carbon might be produced by the action of the rays on any of the elements which are included in the same group as carbon. This work has not been repeated, but the earlier failures seem to have led to a general opinion that the carbon in this case was derived from the grease used for the taps ; a liberation of carbon compounds from this source has often been observed. Mme. Curie, reviewing this work in her treatise, concludes "*on ne peut considérer qu'il y a pas encore actuellement de raisons suffisantes pour admettre que la formation de certains éléments puisse être provoquée à volonté en présence de corps radioactifs,*" and Professor Rutherford, the only other authority of equal eminence, employs similar language.

In the more recent experiments the method of attack has been somewhat different. When an electric spark is made to pass through a gas at a low pressure, rays, very similar to those emitted by radioactive substance, traverse the gas and strike the walls of the vessel in which it is contained. Accordingly if a disintegration of the atoms of elements may be caused by the α and β rays it is possible that it might take place also under the influence of the electric spark. Now it has been known for some time that, when such a spark is passed through the residual gas in a vessel very nearly completely exhausted, considerable quantities of hydrogen make their appearance, although every effort is made at the outset to remove hydrogen and all its compounds from the apparatus. The phenomenon, though it is the cause of many manipulative difficulties, has never been investigated very thoroughly ; it has generally been believed that the hydrogen is liberated from the solid walls of the vessel (and especially the metal "electrodes" between which the spark passes), having been previously absorbed or occluded in those solid bodies

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in some special manner which makes its removal by ordinary agencies, such as heat, extremely difficult.

The experiments of Messrs. Collie and Patterson (together with some earlier work by Sir William Ramsay) have added to our knowledge of the phenomenon three important pieces of information.

(1) Hydrogen is not the only gas which makes its appearance in these circumstances; the gases helium and neon also appear. (2) The relative amounts of helium and neon appearing depend to some extent upon the nature of the gas through which the spark passes. (3) These gases cannot be liberated from the electrodes or the glass of the vessel by any long-continued and intense heating.

It is these discoveries which gave rise to the sensational statements in the daily press which are the direct cause of this article. It is scarcely necessary to say that there is no evidence that the discoverers were responsible for those statements. When they read an account of their work before the Chemical Society they appear to have indicated their opinion that these results were important evidence in favour of the view that the gas appearing so mysteriously is created by a transmutation of the elements of the gas originally present in the vessel, but in the printed account which appears in the *Transactions* of that Society they do not pretend to offer any explanation of them whatsoever. But since those statements have appeared it will be useful to examine how far the new information offers any greater evidence of the transmutation of the elements than the old.

(1) makes it more difficult than formerly to believe that the gas liberated was previously contained in the solid vessel because the known chemical nature of helium and neon renders improbable in their case any kind of absorption with which we were hitherto acquainted. (2) seems to indicate that the new gas comes from the gases previously present rather than from the solids; and absorption of one gas by another is quite unknown. (3) tells against the absorption hypothesis, for all gases absorbed in any manner known hitherto can be liberated by heat. It may certainly be concluded that the phenomenon can no longer be attributed to any action which was previously known and that the hypothesis of transmutation does offer an explanation of it which is not inconsistent with anything known from other sources.

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On the other hand it must be pointed out that the evidence for the view that the appearance of the helium and neon is due to transmutation is not nearly so conclusive as it was in the case of radioactivity. Returning again to the necessary propositions of p. 327 we note that while there is no doubt in this case that the new substances appearing are elements, there is no direct evidence that they are derived from elements. The evidence in the case of radioactivity that the new substances were produced from elements was (1) that the rate at which they were produced depended simply on the amount of the old element present and (2) that the old element disappears at a rate proportional to that at which the new appears ; in the case we are considering, no quantitative evidence of this kind has been offered. The alternative hypothesis is still open that the helium or neon was produced from some kind of compound, different from any with which we were previously acquainted, which was originally present in the vessel or introduced with the gas and cannot be resolved by any means except that of the electric spark. The subsidiary evidence against this hypothesis which was noted in the case of radioactivity is altogether absent.

These are not the only (or even perhaps the most obvious) arguments which can be urged against the necessity of introducing the hypothesis of transmutation to explain Messrs. Collie and Patterson's results. They are given to show how completely the evidence which has been produced differs from that which we considered before. The results are certainly new, they cannot be related at present to any other known phenomena, and if the information which has been given were all that is available, no definite arguments could be advanced against the hypothesis of transmutation as an explanation of them. An objector could only point out that it is rash to accept, even provisionally, an hypothesis which all other experiments render improbable, merely because no other can be suggested on the spur of the moment.

But further information is available. Simultaneously with the announcement of Messrs. Collie and Patterson's discoveries appeared the account of some work by Sir J. J. Thomson. He also had found that under the influence of the electric spark, not only hydrogen, but also helium, neon and, apparently, a new gas, not previously known, might be liberated. This liberation he traced to the action

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of the kathode rays (which are those rays produced in the spark which are similar to the β rays) on the solid bodies in the vessel. He also found that the gases could not be liberated by any means other than the action of the kathode rays and the means which he employed were even more vigorous than those used by the other observers. However—and this is the most important feature of the work for our purpose—he found that if the kathode rays of great intensity were allowed to act for a very long time on a very small piece of the solid, the liberation of the gases ceased and no more gas could be obtained.

There is no discrepancy between the two series of observations. Messrs. Collie and Patterson acted upon a very large amount of solid with a relatively very feeble beam of kathode rays ; it was not to be expected that they should observe the stage at which the evolution of the gases ceased.* But of course if only a limited amount of the helium and neon can be produced while the amount of the other elements present in the vessel is apparently undiminished, it is very difficult to believe that the former are produced simply by the disintegration of the latter under the action of the spark. The alternative hypothesis is far more probable, that the gases were produced from some substance previously present in very small amount in the solid from which they were liberated. It is still, of course, open to the supporters of the transmutation hypothesis to suggest that transmutation ceases for some reason after a certain stage is reached, but they will have even greater difficulty than before in convincing others of the validity of that hypothesis when it has to be cumbered with this addition.

But the question still remains, What is the state in which the gases were present in the solid from which they could be liberated only by the kathode rays ? Sir J. J. Thomson has whisperingly and with all scientific caution advanced a suggestion. Suppose that the "ordinary" elements are faintly radioactive and disintegrate slowly, producing small amounts of new elements. It is known that the speed with which the α rays are projected from the disintegrating atom is less the more slowly the element disintegrates

* The difference in the gases evolved with the nature of the gas through which the spark passed can be attributed to a variation in the nature of the kathode rays with this factor.

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and the less radioactive it is. According to this rule an element as slightly radioactive as the "ordinary elements" must be since their activity escapes our detection, even when it did send out α rays, would send them out with a very small speed; this speed might be so small that the helium atoms, forming those rays, never broke really clear from the remainders of the disintegrating atoms; in order that they should break away completely and appear outside the substance in which they were produced they might need another jerk, such as might be given by the impact of kathode rays on them. Can it be that the limited quantity of helium which is liberated by the kathode rays represents the quantity which has accumulated in the substance by its slow disintegration through past ages, but needs a little extra jerk to free it completely from that substance?

The suggestion is very attractive, but little more can be said of it at present; there is no other evidence for or against it. If the liberation of helium could be explained in this manner, the presence of other gases would present no difficulty, for we have no reason whatever for believing that helium rather than any other element must always be one of the products of disintegration; it is so in the few cases we have been able to investigate; for all we know, neon rather than helium might be produced in others.

But, it may be said, is not this hypothesis equivalent to that of transmutation; is it not mere quibbling to say that the kathode rays do not produce transmutation when you admit that in these cases the results of transmutation can only be made evident by the kathode rays? The question is indeed one of words; if anyone chooses to call this new phenomenon, explained on this hypothesis, transmutation, nobody can stop him. But the term will be very unfortunate, for the phenomenon will have none of the interest, scientific or commercial, which might attach to a change in the nature of atom produced artificially. Scientifically the phenomenon would represent merely another aspect of radioactivity and its discovery would make no new change in the theories of either physics or chemistry. And its commercial interest would be still less. Suppose that it were found that gold could be liberated from lead by this process; the amount of gold which could be liberated would depend not on the intensity of the kathode rays employed but

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simply on the rate at which the spontaneous change in the lead took place. At the present time only that amount of gold could be extracted which had accumulated during the past history of the lead, and before a similar amount could be extracted again a period, probably thousands of millions of years, equal to that during which the lead has already been in existence, would have to elapse. But how much should we expect this amount to be? We can make a rough estimate from known facts. During the past history of minerals containing the radioactive element uranium something less than one part in a million of that element has disintegrated; the rate at which lead disintegrates must be less than one ten-thousandth of that at which uranium disintegrates, for if it were greater lead would have a measurable radioactivity. Accordingly at the present time we might be able to extract from lead not more than one ten-thousand-millionth of its weight of gold, or about one pennyworth of gold from a thousand tons of lead. Even an alchemist might employ his time more profitably than in accomplishing such "Transmutation."

And now I fear that the reader has found the second part of this article as dry as the first. It is not only my fault; the phenomena which we have been considering are not especially interesting even to the professed physicist, still less to the layman. The truly interesting advances in science never reach the knowledge of the journalist, for they are not sensational and have no commercial applications. Perhaps I have abused the journalist enough; but some of us are waiting, with little hope, to see whether any of the more reputable periodicals, which made much of the Transmutation Sensation and published interesting articles on its influence on religion, philosophy and economics, will show that they have still some care for truth by informing their readers, at the least, that all scientific men do not agree with their interpretation of these observations, and that an account has been published of other experiments which do not confirm it.

SOME THOUGHTS ON THE STATE PUNISHMENT OF CRIME

By Sir Bryan Donkin, M.D., F.R.C.P.

THE growing bulk of writings and discussions concerning the causation of crime and the principles and practice of punishment is both a sign of increased public interest in these questions and a source of difficulty for those who aim at a clear understanding and practical grasp of them. This difficulty arises not only from the conflicting conclusions of disputants, but also from the frequently indefinite or equivocal use of the very words that denote the main subjects of debate. Nor is this all the trouble. Even when these terms are clearly stated many writers base their arguments on discordant doctrines or assumptions concerning such matters as the genesis of crime and criminals, the meaning of "responsibility," and the State's object, or even its right, in punishing wrong-doers. Thus the handling of the whole subject tends to become widely discursive, involving consideration of multiform questions, biological, sociological and psychological, and sometimes landing and leaving the student in the dark arena where the spectres of "Free-will" and "Necessity" wage their eternal war.

Now, without denying some degree of relevancy in, perhaps, all these problems to the questions of what persons ought to be coerced for misconduct, or, in other words, whom we are to call "criminals," and if, why, or how they are to be "punished," it seems that the practical relevancy of some of them is very slight. It is remarkable that the conclusions of many penologists are often closely similar while their assumed principles are widely diverse; and it is highly probable that many misconceptions and grounds of dispute would vanish on the attainment of greater precision in the meaning of some

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of the chief terms employed. Such reflections as these have long been forced on the present writer by personal experience in prison administration and individual study of many prisoners, as well as by the reading of numerous books on Crime and Punishment ; and the main intention of this article is to contend, not only for a clearer definition of the terms used, but also for a more practical limitation of the field occupied in current controversy concerning the State Punishment of Crime.

The author of a recent and highly instructive book, entitled "*The Rationale of Punishment*" * is evidently so deeply impressed by the misleading effects of much of the current literature of this subject that he opens his Preface thus :—

"At the present time when the State punishment of crime is constantly cited before the tribunal of Science in order to show why it should not be eliminated, like other relics of barbarism, from the arsenal of modern civilization in which there is no room for mere superstitions of the past, a critical investigation of the problem of punishment cannot be out of place."

Although this statement would, I think, be more accurate were the words "sentiment and sciolism" to be substituted for "Science" in the designation of the "tribunal" that is alluded to, its general truth and point are undeniable. A large part of the pabulum now readily absorbed by the public from books and newspapers relating to this subject tends to favour certain academic doctrines and humanitarian beliefs which question or deny the right of the State to punish law-breakers. Such literature has its effect on both the general and the criminal public. Some writers teach that crime is the outcome of what is called a vicious social "system," while others regard "criminality" as a malady of individuals ; but many from these two extreme groups either imply or explicitly state that the actual "criminal" is not responsible for his crimes, and that either "Society" or "Nature" makes the criminal. In a book about crime and punishment, written not long ago with apparent gravity by a popular novelist, it is categorically stated that "imprisonment as a punishment has failed" ; but when denying that the punishment of flogging had put down garrotting the author argues that this

* *The Rationale of Punishment*, by Heinrich Oppenheimer, D.Lit., LL.D., M.D., University of London Press.

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crime was stayed by the "ordinary law with its ordinary punishments"! Again, a prisoner, pronounced to be sane by a medical expert, pleaded recently before a court of law, in excuse of punishment for a crime which he admitted, that "all men were irresponsible and unable to help what they did," thus voicing accurately and carrying to its full logical extent the necessitarian doctrine of the "born criminal" which implies, of course, the equally natural and inevitable production of the crimeless man. Once more, a convict, versed in the literature of crime and its genesis assured me in an interview that all his misdeeds were due to the inheritance of his "constitution" from a drunken father, and that he was therefore irresponsible; while another habitual criminal informed me that society manufactured the criminal and consequently should answer for him. Thus we see that authors both of crime and of books on crime may hold widely different opinions on the origin of crime; but that these discordant opinions, while crediting such origin either to destiny or to the fault of society (or, to use the now popular but misleading phrase of the day, either to "*Nature*" or to "*Nurture*"), make mutual truce in teaching the illogicality and injustice of punishing the criminal. Even these brief instances of current opinions may serve to justify the following remarks on the import of such terms as "criminal," "punishment," "responsibility," etc., and the necessity of some common accord in our use of them.

The words "*crime*" and "*criminal*," employed as they often are, without precision of meaning, cause much confusion of thought both in teachers and students of this subject. A thoroughly bad man is often called a criminal, whether or not he has broken a law; while a law-breaker, convicted and punished for a serious offence is not infrequently spoken of, even by experts and officials, as "not really a criminal." The more our minds are intended on the question of what we deem should be the proper treatment of an individual offender, the more liable we are to use the word "criminal" in this popular or moral, or, as some say, "subjective" sense. It is true that perhaps most offenders dealt with by the State are criminals both in this sense of the word, and in the legal and correct sense of "law-breakers." But in all questions concerning the State punishment of Crime it is surely of the first importance to restrict the

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meaning of the words "crime" and "criminal" to that of offences and offenders against the existing laws of contemporary society. Whatever views may be held as to the genesis or production of wrong-doers, individually or collectively, all should agree in the use of the substantive "criminal" as denoting a "law-breaker" only, irrespectively of the nature and quality of the law-breaker or the law which has been broken.

In the remaining remarks concerning criminals as law-breakers, the following assumptions are made, which cannot be discussed within the limits of this article: First, that the State or Society in its corporate form is justified in coercing or punishing law-breakers; and, second, that the word "law-breakers" denotes such members of society who transgress rules sanctioned by society itself for its own protection. This second assumption will be dealt with somewhat more explicitly below, in considering *punishment*; but it does not seem necessary to enter into detail concerning the precise motives and methods of the State in dealing with criminals. It is enough to understand that these motives have been and will probably continue to be of an indefinite, mixed, and changing character, and that the methods of treating criminals must be modified accordingly from time to time.

As a corollary of what has been just said, it follows that law-breakers, speaking generally, are responsible for their offences, or, in other words, as will presently be seen when the topic of criminal responsibility is considered, are rightly liable to punishment. With such as hold that no ill-doer can help his ill-doing and consequently that well-doing is equally unconcerned with the will of the well-doer, no argument is needed by the requirements of the subject in hand. The ultimate dispute about free will and determinism no more concerns us here than is the actual conduct of most sane men in any way influenced by the views they hold on the meaning of Causation and on the other speculative questions. In practical life men act on the assumption of their ability to choose their lines of action, of the "reality" of themselves and of other men and of the external world generally, and also of causation as meaning something more than a mere sequence of sense impressions; and they act on these assumptions however tenaciously they may adhere, when at their writing desks, to opinions which conflict with their everyday words

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and deeds. The postulate of practical freedom of action underlies of necessity all social existence, and all notions of praise or blame. Denials of this proposition, with which no determinist need disagree, should give no trouble to the penologist.

From the definition that has been adopted it follows that *crime* is manifold ; and that *criminals* form a motley group, including all law-breakers, both convicted and undetected, sane and insane : guilty of offences varying from acts of indecency and neglect of bye-laws up to deliberate rape, slow poisoning, or long-planned fraud. At least for practical purposes all men must be regarded as potential law-breakers, even as they are subject, and often yield, to temptations of postponing all consideration of the general welfare to the satisfaction of their individual desires in many matters where their actions would neither be criminal nor even regarded as criminal in the popular sense of that word. There is no "criminal type," and any formulated doctrine of hereditary crime must be gravely misleading. Many persons commit even serious crimes to whom but little social blame attaches ; and it seems certain that most young people could be readily trained, in appropriate circumstances, to be habitual criminals of one kind or another, whatever their innate capacities or tendencies might be. But, as will presently be seen, there are many criminals with innate mental defect, signalised prominently in their conduct otherwise than by the commission of crime, whose criminal careers can justly be attributed to this defect and to their consequent incapacity of being *socially* trained, as most people are trained, to resist their individual desires or other anti-social influences from without. It is with regard to this class of criminals, indeed, that the important question of criminal responsibility is very frequently raised.

Every man is the necessary result of his innate organic capacities and of all subsequent influences which may develop, restrict, or destroy them. There seems to be no more a special "problem" of the genesis of "criminals" as a group, than there is of the genesis of virtuous and law-abiding persons. For his proper and necessary function as a social and still more as a civilized animal man must possess, besides the innate structure of brain which underlies his great capacities for making acquirements, the equally necessary opportunities for learning to make those acquirements. A man,

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as has been said, cannot practise morality on a desert island. It is equally true that were a human being to be nourished from infancy to maturity without human association, he would not only fail to develop "morality," but also, whatever were his inborn capacities, would be speechless and devoid of all detectible mental characters that are distinctively human. His only *differentia*, in comparison with an imbecile, would be that, given an average brain at birth, he might be educable in average human degree; and by *educable* is meant here *responsive* to the myriad influences of that multiform conversation with the environment which affects human beings during their lives. For a full and instructive exposition of this part of the subject, which has by no means unimportant bearings on the endeavour to arrive at a correct and common understanding of the term "criminal," the inquiring student is referred to Archdall Reid's original work on the *Laws of Heredity* and especially to Chapters XIX.—XXIII.

In considering, next, the meaning and use of the term *Punishment* in connection with the subject of the State coercion of criminals, it must be borne in mind that at least one, though not the only essential object of the State's dealing with criminals is, at the present day, the protection of society by the detention or deterrence of actual criminals, and by the prevention, by means of threatened and certain penalties, of the commission of crime by others. It will thus be seen that, in this context, the words "punishment" and "crime" imply one another. In the work of Dr. Oppenheimer, which has been quoted above, the statement that punishment is the "social reaction against crime," is objected to on the ground that it is a "circumlocution rather than a definition." If by this is meant that for the purpose of explicit definition this phrase is imperfect: that it is rather an interpretation or a description than a definition, and that to the words "social reaction against crime" should be added "expressed by the infliction of pain, or suffering, on the criminal" (or other words to this effect), I am in accord with him. But if the author means by "circumlocution" a tautological or otiose statement, I disagree; and indeed Dr. Oppenheimer admits, in this place, that the statement he quotes is valuable because it draws attention to the important fact that the terms "Crime" and "Punishment" connote one another. It is clear, therefore, if the

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“State punishment of crime” is taken to mean the infliction of suffering by contemporary society on its members who transgress rules enacted and provided with sanctions by itself for its own protection, that the field of criminal law is thus definitely distinguished from that of positive morality, and also that there is no necessity, in order to arrive at a satisfactory definition of punishment, to enter upon an exhaustive history or consideration of the various theories of “principles” of punishment that may have been propounded or acted upon by various societies in the course of ages.

The word “punishment” itself connotes both the infliction of some kind of suffering and also the notion of retribution as a motive element. This is really the current as well as the correct use of the word, in whatever relation it may be employed. The element of retribution, retaliation, requital—the sense of justice in the offender’s suffering, enters essentially, though often not very prominently, into the concept of punishment, judicial, social or individual. Many writers, however, fail to admit that if even the most humane of modern societies neither justified nor desired some kind of retaliation on the individual criminal, their very humanity would disallow some of the severer sentences now generally approved as necessary for the safety and welfare of the community. And some who make this admission seem often to forget that, as a matter of fact, the infliction of suffering must inevitably be an element in every measure that involves the coercion of criminals, without any respect to the expressed or implied object of such coercion.

If this view be accepted, it follows that those who hold the sole admissible object of the State in dealing with crime to be the Reformation of the criminal, or, as they often, but very oddly, phrase it, that the Reformation of the Criminal should be the only object of “Punishment,” must either explicitly admit that the infliction of suffering is a necessary instrument of reformation, or should explicitly disallow it altogether and drop the use of the term punishment. The very loss of liberty which, apart from certain exceptions, is almost an integral and certainly a most important item in all systems of State dealing with law-breakers is the chief “punishment” now employed in civilised countries, and is truly punishment in the literal and correct sense. Doubtless it is regarded as such by the sufferer, who ignores the motives of those who incarcerate him.

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Few, indeed, will deny that the threat of this punishment—the loss of liberty—coupled with the certainty of its infliction on the detected criminal, prevents the commission of countless crimes. Even empty prisons would still be powerful deterrents. The risk of immurement would preclude the actuality of potential crime even in the futurist community hazily imagined in certain philosophers' dreams which prophesy the biological evolution of some new "Sense of the State" by the operation of "Natural Selection."

In connexion with these remarks on the meaning of "punishment" I would insist here, in order to avoid misunderstandings, that the term "State punishment of criminals," as interpreted above, by no means covers the whole function of the State in dealing with criminals, nor is in any way incompatible with the duty of the State, now generally recognised, to make all practicable efforts in the direction of reforming those whom it necessarily punishes. Although Reformation cannot be the primary aim of the State in dealing with criminals, unless it be held that the State, as a Super-parent, should strictly control the lives and upbringing of all and thus aim at the prevention of all misconduct, there is yet a wide and fruitful field for the State's work in striving to reclaim criminals. In the light of present knowledge and feeling the State is rightly bound, even if only with the object of the prevention or limitation of crime, to use all possible means to compass this object, especially in the case of such reformable offenders as many of the younger criminals are known to be. Whatever views be held about "necessity" and "moral responsibility," or whether we philosophise from the "animistic" or "mechanistic" standpoint, we may certainly accord with the belief in the great educability, at least in the young, of those still developing cerebral structures which are admittedly necessary to all mental expression and to the proper ordering of human conduct. There is already practical proof of the efficacy of strenuous public endeavour in this direction, as evidenced by the records of the work done at the "Borstal Institutions"—the State establishments for the reformation of adolescent criminals,—and also of the valuable efforts of the philanthropic "Borstal Association" (which labours in co-operation with the State) in encouraging and successfully assisting the inmates of these institutions after their release.

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It remains now to enter as briefly and warily as possible into the practical aspect of the much-disputed question of *criminal responsibility*—that maze of dispute where mutually hostile philosophers and ever differing jurists and physicians pursue each other unceasingly. We have already cut this Gordian knot by assuming, as a working hypothesis, be this a delusional procedure or not, the practically free agency of most men, including criminals. It is necessary for our purpose to assume further that the conviction and treatment by the State of actual law-breakers should be regulated and adapted with regard not only to the degree of harm or menace to society involved in the offence, but also, as far as possible, to the character and conditions of the individual law-breaker. This assumption is based on the recognition of the fact that modern humanity in most civilised countries practically disallows the infliction of more suffering than is necessary or expedient. The "individualization of punishment" is now more or less formally recognised as a goal to be striven for by modern penologists.

The present criterion of legal responsibility, by which the punishability of a man for a proved offence is decided, may be looked upon crudely as an answer to the question whether or not he could help committing the offence; and the silent assumption is that he could help it unless strong reason to the contrary be adduced. Unless a definite plea of permanent or temporary insanity be put forward and established, the accused is regarded as duly "responsible." When insanity is pleaded, the practice is to leave to the jury the question whether the accused had a sufficient degree of reason to know he was doing an act that was wrong. To establish the defence the accused must be so insane as either not to *know* the nature and quality of the act he was doing, or, if he knew this, as not to *know* he was doing wrong.

It has been shown by Dr. Mercier, in his carefully reasoned work on Criminal Responsibility, that the criterion of responsibility even as interpreted above, is imperfect. Not only should the nature of the act, and of its wrongness, be known by the accused, but also there must be knowledge and appreciation on his part of the circumstances in which the act was done; and, further, in assessing "responsibility," it must be remembered that a person may know

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his act is wrong without knowing how wrong it is. But in spite of this it would appear that, at least in the matter of serious crime, the requirements of both justice and mental science are sufficiently satisfied by an enlightened application of the existing criterion of responsibility in cases when insanity is pleaded as a defence. Nevertheless there is a large class of cases in which the plea of defective or disordered mind is either not set up at all, or is not established to the satisfaction of the Court as coming up to the due criterion of responsibility, where the accused is, without doubt, so defective in mind as not to be fully "responsible" in that sense of the word which we are now considering.

In the outset of Mercier's work, to which reference has just been made, the late Sir James Fitzjames Stephen is quoted as follows :— "In all crimes the definition consists of two parts, the outward act, and the state of mind which accompanies it." "Thus," says Mercier, "all crime is in part a problem in psychology." Stephen justly complained that while, to the judge, the word "responsible" meant liable to punishment by the law as it *is*, to the medical man it meant something different, *i.e.*, probably, "liable to punishment by the law as it *ought to be*." According to Mercier, the meaning of the word "responsibility," in this context, should be "the quality of being rightly liable to punishment"; and he explains that he intends by this, "liable to punishment on grounds that commend themselves to be equitable and right to the common sense of the common man of this time and country; or, in other words, that appear to be just and fair to the ordinary man when they are explained to him." This interpretation of the word is here adopted. There should be nothing startling in it to thoughtful jurists or psychologists. It is virtually the basis of most laws in contemporary civilised societies; and we know that Law, though rightly changing more slowly than Opinion, must in the long run, be moulded by Opinion.

For a full discussion of "criminal responsibility" the reader must be referred to Mercier's original work. Only his summing up, after a detailed and clear exposition of all the terms used in his argument on this question, can be quoted here :—

"Responsibility attaches to acts that are wrong—a wrong act is a *voluntary* act in which the actor seeks gratification by inflicting

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inadequately unprovoked harm upon others. Responsibility is the more undoubted the more deliberately, the more frequently, the will is concerned in the act."

I believe that this definition is generally applicable in the case of all law-breakers, including of course such persons as are often and vaguely called political offenders and others who do serious harm to society in the professed and sincere belief that they are serving a religious or other ideal object. But there is an important class of law-breakers, now habitually tried and sentenced as fully responsible or rightly liable to the punishments awarded to sane persons, to whom a better criterion of responsibility than is now recognised by the law would be especially applicable. These, although not certifiable under the lunacy laws are the subjects of marked mental defect, and among these are many habitual and dangerous criminals who have in all probability been thus defective from their birth. The prevention of much of the crime now vainly and expensively dealt with by the ordinary penal laws would undoubtedly be secured by the early detection and control of this class of persons who now contribute so considerably to the population of our prisons.

It may be admitted that in some cases apparently includible in this class there might be some difficulty in arriving at a just conclusion ; but it would usually be easy for a thoughtful student of sane and insane men to grade the responsibility, after a careful investigation and consideration, of each individual case, and after taking into account *their general conduct, apart from the special crime charged*. In this matter, surely, the careful and experienced psychologist can be of real service to the practical penologist both in saving these mental defectives from the infliction of greater suffering than is incidentally necessary for their detention, and also in contributing to the social welfare by aiding in the early detection and detention of this class of cases, and thus preventing their accession to the actually criminal ranks. I am convinced, from personal experience, that the observant study of many persons of this class who are charged with crime would render practicable their certification as at least not fully "responsible," or, to use the French term, as of "attenuated responsibility"; this decision being based on the ground of sufficient evidence drawn from their general conduct, irrespective of the particular crimes in question,

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that the offences with which they are charged were not, in the fullest sense, *wrong* acts, according to the definition accepted above. One of the most important results of the passing into law of the "Mental Deficiency Bill," will be the legal recognition of grades of responsibility that are now ignored, and a consequent diminution of crime as well as a better and more rational treatment of many irresponsible criminals.

I confess to the opinion that the practical problems of Crime and Punishment are neither so many nor so deep as they are often represented ; and that it is a vain effort for the penologist to strive after the discovery of such first principles of punishment as will apply to all countries at all times. In this sphere at least it would seem that adherence to what is called the Philosophy of "Pragmatism" would work well, and serve to eliminate much fruitless disputation that now overloads the literature of the subject, as well as to harmonise and satisfy the equally vital claims of the social senses of humanity and justice. I cannot greatly regret what Dr. Oppenheimer, in his *Rationale of Punishment* terms the "meagreness" (or, as I take it, the *scarcity*) of English literature on crime and punishment as compared with that of some countries ; but, while freely admitting that we have, for instance, no comprehensive treatise in the English language which approaches in scientific excellence and practical value to the "*Philosophie Pénale*" of M. Tarde, I would claim very high places in the serious student's estimation of works on some of the aspects of this subject, for the original books by Mercier and Reid from which I have quoted above, as well as for the notably thoughtful and learned essay by Dr. Oppenheimer himself.

VITALISM AND MATERIALISM

By Charles A. Mercier, M.D., F.R.C.P.

IN this controversy I hold a detached position. I am neither Vitalist nor Materialist, and I hold, and think I have proved according to the strict canons of logic, that the problem that each set of antagonists tries to solve, and claims to have solved, is completely insoluble. I hold no brief, therefore, for Dr. McDougall, who is well able to take care of himself, but when Mr. Hugh Elliot flaps his wings and speaks in the voice of chanticleer, when he speaks of the "slobbering inefficiency" of those who do not think as he does, and says they "dwell in a heavily-scented and unwholesome atmosphere of lies," at the same time that he claims for himself and for those who think with him a monopoly of the desire for truth, the bile of the natural man is stirred, and his bowels yearn to show that this mode of controversy does not lead to conclusive results.

When anyone asserts a positive doctrine, on him lies the *onus probandi*; and his proofs or his evidence stand to be shot at by all comers. Nothing is easier than to bring destructive criticism to bear upon any hypothesis of the connection of mind with matter, and as long as Mr. Elliot is attacking Dr. McDougall's doctrine of Vitalism I have little difference with him, though I think some of his expressions go beyond the limits of fair controversy, and therefore of effective controversy. But Mr. Elliot is not content with a purely destructive criticism; he sets up a counter-hypothesis, and though this hypothesis is not the child of his own loins, he seems as proud of it as if it were, and is even boisterous in his assertion of its truth and sufficiency. This is his weakness. The *onus probandi* is now shifted, and lies on him. It is he that now stands to be shot at, and I propose to send a few missiles in his direction.

In the third number of BEDROCK (for October, 1912) Mr. Elliot lays down twelve propositions, each of which, he says, except the

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last, is non-contentious, and represents an established fact. I venture, however, to contest some of them, though I admit that in my opposition it is quite possible that I stand alone. I am not dismayed by this, however, for I have seen other doctrines generally adopted that I once held in solitude.

His fifth proposition is that—

“As in the past, the sole evidence offered for them [animate causes] is that mechanical explanations have not yet been proved.”

“(6) No direct evidence of any kind has ever been found for the existence of a vital force.”

“(8) The whole nervous system is built up on the reflex principle, and forcibly suggests mechanism.”

“(10) Vitalism involves a creation of energy or of matter. It is proved that neither takes place.”

I take upon myself to deny every one of these four propositions, either in whole or in part.

As to his fifth, Mr. Elliot here admits that for some occurrences no mechanical explanations have yet been found; and he will scarcely deny that mechanical explanations have been diligently sought for them. It would seem, therefore, that with respect to these occurrences the proper philosophical or scientific attitude would be, not to assert dogmatically, as Mr. Elliot does, that they must be and are due to mechanism, but to suspend the judgment until the matter is proved one way or the other. It is a plausible argument that since mechanism now accounts for so many things that were once credited to animistic causes, therefore it will some day be found to account for the residue of things that are still credited to animistic causes—it is a plausible argument, but it is not a conclusive argument. Astronomers may have arrived, and, I think, did arrive, on similar evidence, at the conclusion that all planets and satellites thenceforth to be discovered would be found to revolve round their primaries in the same direction; but they would have been wrong, and Mr. Elliot may be wrong. It would be wiser of him to wait and see.

“No direct evidence of any kind has ever been found for the existence of a vital force.”

This is clearly erroneous, and, I think, Mr. Elliot must admit upon reflection that it is erroneous. If he had said that there is no

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positive proof, one could have admitted his contention ; but when he says there is no direct evidence *of any kind*, he goes far beyond the facts. Every exertion of the will that is followed by a bodily act or movement is evidence for the existence of a vital force. I do not say that it is irrefragable proof ; but incontestably it is evidence, and evidence that cannot lightly be dismissed. Indeed, I think it is quite fair to say that it is sufficiently cogent evidence to enable the hypothesis of Vitalism to hold the field in this particular region until the hypothesis is disproved. And it has not been disproved. Doubts have been cast upon it ; evidence has been accumulated against it ; but it has not been disproved. To the unsophisticated mind nothing appears more certain than that the mental operation of the will is the cause of the material movement. To the mind of the trained psychologist it is known that this very sequence is the foundation of our notion of cause and effect, whether it is in fact cause and effect or no. To say that there is no direct evidence of any kind of the existence of a vital force is therefore, without doubt, erroneous.

“(8) The whole nervous system is built up on the reflex principle, and forcibly suggests mechanism.”

I do not see that the reflex principle is any more or any less mechanistic than the principle of autogenic movement. A steam engine or a gas engine is not built up on the reflex principle ; it is built up on the autogenic principle, but it is not on that account any the less a mechanism. Whether the whole nervous system is built up on the reflex principle I am not concerned to admit or to deny, but I do very strenuously deny that its action is wholly reflex. I know quite well that all nervous action is generally supposed to be either directly or indirectly reflex, and to be governed by the reflex principle ; but, speaking for myself alone, I hold that this view is profoundly erroneous ; and I have discussed the matter at length in my book on Conduct, which is in great part founded on the view that autogenic action is as much a property of the nervous system as reflex action, and on the whole is more important.

“(10) Vitalism involves a creation of energy or of matter. It is proved that neither takes place.”

Both of these propositions are plausible ; neither is necessarily

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true : the second is certainly untrue. Mechanists make great play with their notion of the "closed circuit" of matter and motion. Sense organ, afferent nerve, grey matter, efferent nerve, and muscle, constitute, so they say, a closed circuit along which motion travels, and the intervention at any point of a directing mind must necessarily mean the creation of energy that was previously non-existent or the annihilation of energy that previously did exist. The latter alternative is not mentioned by Mr. Elliot, but it is plainly a necessary alternative. This is really the strong position of the mechanists, and it must be admitted that it is a very strong position, and to persons with the preconceived ideas—and may I say the prejudices—of the mechanists it is an invulnerable position. It is on the ramparts of this fortress that they clap their wings and crow. But is the position after all so completely invulnerable ? I must confess that for many years I shared the preconceived opinions and prejudices of the mechanists, and was almost as cocksure as Mr. Elliot that the fortress was unassailable ; but on a critical survey of the defences I find two places in which a breach is practicable. The mechanists assume that the circuit is closed ; but it is a pure assumption. How if the circuit should not be closed ? What then becomes of the impregnable fortress ? It is a castle of Edward III. attacked by modern artillery. Suppose that at the turning point the circuit is not closed, but that here a modicum of the inflowing nervous current escapes—I am not saying that it does or does not, I am merely making the supposition—and that, having escaped, it remains in store in that position, ready to interfere and change the operation of the closed circuit on occasion. So far I do not think the most bigoted mechanist—granted that there is such a thing as a bigoted mechanist, which I am inclined to suspect since reading Mr. Elliot's articles—I do not think the most bigoted mechanist would object : but, stop a bit, I am going to make his flesh creep. Suppose that, at the gap in the circuit at which the energy escapes, it is forthwith transformed into some form of mind—say, into latent will—and that, when the latent and potential will becomes actual, there is a retransformation of mind, that cannot act on the closed circuit, into energy, that can. How now ? Here is a Vitalism that needs no creation of energy or matter, and Mr. Elliot's tenth non-contentious proposition is invalidated, and his established fact is

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no fact, but an hypothesis that needs to be established. I think I hear the mechanists furiously rage together : " What you have supposed is inconceivable ! Energy cannot be transformed into mind, and we have not even the rudiment of a faculty that would enable us to imagine such a transformation ! " How true ! Energy, which is a purely mental concept, cannot be transformed into mind—because it is mind already—and that which we cannot conceive can in no case be true. The action of one body on another at a distance, for instance, is quite inconceivable, and therefore gravitation cannot be true. Have you fallen down and hurt yourself ? Nonsense, my child ! I cannot conceive how the earth could attract your body, and therefore you cannot have fallen.

But if the mechanist is an acute mechanist, he has a better answer than this. He may say, and say justly, " When you speak of energy being transformed into latent will, and the latent will, on becoming actual, being retransformed into energy, you are merely juggling with words. The energy remains energy, though you call it by another name, and the will never really acts at all." A good criticism, a fair criticism ; but, alas ! is not all this pother about pretending to solve an insoluble problem a juggling with words ? When you seek to determine the relations of mind and matter are you not virtually trying to determine what takes place when an irresistible force impinges on an immovable body ? Are you not trying to discover the song the Sirens sang to Ulysses ?

Mr. Elliot's tenth established fact is that it is proved that creation neither of energy nor of matter takes place. Is it really proved ? I should be curious to see the proof, and I wonder if it would satisfy me, or any other moderately reasonable person. And how could such a thesis be proved ? What possible evidence could be given that at no time in the long stretch of eternity, and at no place in the infinite dimensions of space, had a particle of matter or a spark of energy ever come into existence ? Again, Mr. Elliot has assumed hastily and inconsiderately the *onus probandi*. I do not assert that energy and matter can be, or have been, created. I make no assertion at all either way. It is he that takes upon his shoulders this monstrous burden, and assures us that *it has been proved* that neither energy nor matter is ever created. Well, if it has been proved, let the proof be forthcoming, and let it be a real proof. Let

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him not fob us off with some quasi-proof, such as the assertion that no man has succeeded in creating either ; and let him understand that such an assertion, if it be made, is, firstly, no proof of what it asserts, and, secondly, begs one of the very questions at issue, viz., whether an exertion of the will is not in fact a creation of energy. I do not say it is : I do not say it is not. What I say is that Mr. Elliot does not prove the fact either way by making an assertion about it.

Mr. Elliot is a reasoner of considerable power, and one would have thought that he would not be likely to fall into an elementary fallacy—a fallacy so manifest and so transparent that even Aristotelian logic has been able to detect and expose it. He says that if $a = b$, then $b = a$. This is quite true in mathematics, but he imports it into logic, and in logic it is not true, for logical number is, as I have shown in my book on logic, not the same as arithmetical number. He goes on to translate and apply : he says, “ If matter is feeling, then feeling is matter.” Is it ? Then if money is wealth, wealth is money ; if cloud is vapour, then vapour is cloud ; if ships are vessels, then vessels are ships ; and if an argument is nonsense, then nonsense is an argument.

Mr. Elliot complains that Dr. McDougall has not offered any reply to a single one of the twelve propositions in which Mr. Elliot stated his faith, and he professes astonishment at Dr. McDougall's omission to refute them. Seeing that Mr. Elliot himself posits them all except one as non-contentious and as established fact, I cannot help thinking that his astonishment is more assumed for stage effect than genuine feeling ; and I doubt whether it will not become a very real astonishment now that he finds that some of them are actually questioned. I am afraid that he will attribute this paper to “ those melodramatic and thaumaturgic instincts, which pullulate in every public assembly and in every newspaper,” and I naturally shrink from being accused of such horrors, especially as I have not the slightest notion what he means by them. I beg to assure him that as far as introspection can decide the matter, I am quite innocent of melodramatic and thaumaturgic instincts, whatever they may be ; but as I am neither a public assembly nor a newspaper, perhaps my anxiety is uncalled for. Anyhow, I swear I do not pullulate, nor do my instincts pullulate in me, whatever Mr. Elliot says.

NOTES ON THE STRUGGLE FOR EXISTENCE IN TROPICAL AFRICA

By G. D. H. Carpenter, B.A., B.M. (Oxon.), F.E.S.
(Member of the Royal Society's Sleeping Sickness
Commission in Uganda)

WHEN questions of Mimicry are discussed, arguments are based mainly upon the appearances of specimens in the cabinet, for comparison can then be made between very large numbers of individuals collected at one and the same time under the same conditions, and others collected from different localities. But inasmuch as the whole point of the resemblance of one insect to another often depends upon the appearance during life under natural conditions of environment; a contribution to the discussion upon Mimicry from the point of view of a worker in the field may be not without interest.

The writer, for the best part of the last three years, has been engaged on behalf of the Royal Society, in studying in its natural surroundings the Tsetse fly *Glossina palpalis*, the carrier of Sleeping Sickness, in Uganda. This has brought him very closely into touch with insect life, necessitating prolonged observational work in the haunts of the fly—forests, abounding in insect life and especially with mimetic butterflies.

The greater part of the time was spent on the equatorial islands in the north-west corner of the great Lake Victoria (commonly and erroneously called Lake Victoria Nyanza), from which the former inhabitants had been removed a few years before in order to save them from being utterly destroyed by Sleeping Sickness. The only people

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on the islands were, therefore, the writer and his native servants, canoe men, etc., so that the problems could be studied unmarred by human influences.

One of the most abundant species of butterfly in the forests of Bugalla in the Sesse Archipelago, was the protean Nymphaline butterfly *Pseudacræa eurytus* Linn. This species occurs in several forms in Uganda, all supposed, when first described, to be distinct species. In two of them, the forms *terra* and *obscura*, male and female are alike; in another, the form *hobleyi*, male and female are quite different in appearance. The writer has been able to prove by breeding, however, that all are of the same species. Each of them is a beautiful mimic of a different species of the genus *Planema* belonging to the typically distasteful and protected group *Acraeinæ*. The *Planema*, which is the model of *Pseudacræa eurytus*, form *hobleyi*, was figured in Professor Poulton's article in BEDROCK, Vol. I., No. 1. On the mainland, in the neighbourhood of Entebbe, as shown by the captures of Mr. C. A. Wiggins, the *Planemas* considerably outnumber the mimics, and specimens of *Pseudacræa* intermediate in appearance between two of the typical forms are rare. It seems reasonable to suppose that where the distasteful models are abundant the *Pseudacræas* which resemble them are kept rigidly up to the mark by Natural Selection.

On Bugalla Island, in the Sesse Archipelago, however, a very different state of affairs is found. The *Pseudacræas* abound, but for some reason (very probably scarcity of their food plant) the *Planemas* are extremely scarce, so that the proportion which obtains between model and mimic on the mainland is more than reversed on the island.

Now here is a most interesting and suggestive fact. The *Pseudacræas* are found to vary in a most extraordinary manner, so that one finds every kind of gradation between one form and another. The commonest variety is that coming between the typical form *terra* and the typical form *obscura*; but a large number of specimens show also a blending with the form *hobleyi*, as indicated by a reddish suffusion at the base of the hind wing on the underside, where in the typical *hobleyi* is a strongly marked red triangle mimetic of the corresponding marking in the *Planema* model. Such a marking, easily seen on a distasteful species, has been called an aposeme, a

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danger signal warning off enemies ; and warning colours generally are spoken of as aposematic.

But these are not the only variations ; individuals occur which vary in other directions, and one was caught which approaches very nearly to one of the forms of *eurytus* from the West African coast, there mimetic of another *Planema* which abounds in the same locality. Now what is the meaning of the extraordinary abundance of varying forms on the islands and their rarity on the mainland ?

It is surely reasonable to suppose that the explanation is to be found in the scarcity of models on the islands.

On the mainland, where the distasteful models are sufficiently abundant to have a protective value, a variation on the part of the mimic away from the type would lose the protection afforded by its resemblance to the model, and would be more likely to be destroyed than the type which is there kept rigidly up to the mark.

On the islands, however, the *Planemas* are so scarce that it is quite possible that an enemy of butterflies might never see one ; so that the *Pseudacræas* have nothing to lose by not maintaining their strict resemblance to a model of such scarcity, and variations departing from the type of the model have as much chance of surviving as any other form.

It is as if in this case Natural Selection were rendered inoperative, and the results are extremely striking.

Full details of all the material obtained are in course of preparation. These variable *Pseudacræas* also supply an excellent answer to a difficulty which Professor Punnett appears to feel very acutely, namely, the Selection value of the slightest variations. He alludes in the last number of *BEDROCK* to :

“ this enormous assumption of Professor Poulton's—that a very slight accidental variation on the part of a species, in the direction of a pattern which is utterly different, will be detected by its enemies and will cause them to let it alone.”

This is certainly a difficulty when one studies minute differences in cabinet specimens only, and in truth it does seem an “ enormous assumption ” to suppose that minute fluctuational varieties cause such a change in appearance that the difference can be seen on the wing.

But yet it is so, as any field naturalist will agree. The commonest

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Pseudacraea on Bugalla Island was the form known as *Ps. terra*. The fore wings are of a rich dark blackish brown, with an oblique bar near the apex and a patch on the posterior (inner) margin of the wing of a tawny orange colour.

Having seen large numbers of these on the wing, the writer soon became familiar with what might be termed the average, or typical appearance of this form; and it was extremely interesting to find what a small departure from the type was enough to give the insect a different appearance. Thus, one specimen which he caught and sent to the Hope Department with the note, "Looks distinctly different on the wing," had simply a *very* slight sprinkling of orange scales on part of the black costal (anterior) border of the fore wing, just internal to the subapical orange bar.

Again and again one found that a slight sprinkling of white scales on this orange bar near the apex, or a slight difference in the breadth of the blackish area separating this bar from the adjacent patch of orange, was enough to make the butterfly appear, on the wing, quite different from the type. This may seem very remarkable when these small variations are seen in the cabinet, but it is a fact. Now no one who has seen insect-eating birds pursue and catch their prey, can deny that their acuity of vision must be far superior to that of man. Who has not seen our spotted fly-catcher or, in the tropics, a bee-eater, dart off and catch some insect wholly invisible to the observer? That being so, it is surely not an "enormous assumption" to believe that the minute variations above mentioned, and even variations of less degree, are readily detected by birds.

Further, it must be remembered that, under natural conditions a bird, or other vertebrate enemy of such butterflies, does not have time to consider appearances as one would do with rows of cabinet specimens laid out before one for time unlimited. A butterfly on the wing in the forest is only intermittently exposed to view as it flits about among the bushes and undergrowth amongst its fellows, so that a brief moment of hesitation and indecision on the part of a well-fed bird, owing to doubt whether it has seen that particular form before, would give the insect the advantage of the doubt, and in a very few seconds it might be out of reach or in a bush where the bird could not follow. The writer says "well-fed" bird advisedly because in a tropical forest, teeming with insect life, it seems hardly

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possible that any bird can want for food, so that many insects which might well be eaten under stress of great hunger, will escape owing to their relative distastefulness. It is significant that Mimicry is best shown where insect life is most abundant ; and Mr. G. A. K. Marshall and others have shown that a hungry insect eater will eat insects which it refuses to eat when not very hungry, although it will still eat less distasteful food. He does not think that the most ardent supporter of the Mimetic theory is prepared to claim that there is such a thing as "*Absolute*" distastefulness in an insect—the question of inedibility is entirely a *relative* one. It must be admitted, that a mimic in the cabinet and a mimic on the wing, or under the special conditions of environment upon which the Mimicry depends, are quite different things.

Those who work out museum collections often receive specimens with the field note "A on the wing closely resembles B," and yet when the insects are prepared for the cabinet the resemblance is not so very obvious.

So that the questions of flight and attitude become of great importance. In the famous case of *Papilio dardanus*, the form *planemoides* does not in the cabinet appear to be a very good mimic of the *Planema* after which it is named, and yet the first one that the writer saw completely deceived him, although he had had over two years' experience in the field.

The butterfly was fluttering slowly and heavily over the track through a narrow belt of forest, just after the manner of the model ; and the immediate thought that flashed through his mind was, "What an enormous *Planema* !" He promptly caught the butterfly and found to his great joy—for there is nothing that pleases a field naturalist more than being deceived by Mimicry—that it was not a *Planema*, but a female "swallow-tail."

So strongly had instinct been modified in this butterfly that when captured it still continued to behave like its model, and lay quite quietly in the net, as the *Acræinæ* do usually. The non-mimetic males, however, like both sexes of other non-mimetic *Papilionidæ* which the writer has caught, flutter wildly in the net in the endeavour to get away. The reality of the resemblance of the model to the mimic, in manner as well as in appearance, is most striking.

As has been said, the first impression given was that of a large

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Planema, and had the writer been a bird, with a large variety of insect food at its disposal from which it could select what best pleased it, instead of a collecting entomologist eager to catch anything of interest, a moment of hesitation might well have allowed the butterfly to escape.

For, if thoroughly alarmed, these and others of the not very distasteful mimetic species seek safety in vigorous flight which is very different from the lethargic, "show-off" flight assumed when they are not alarmed and in fancied security from their resemblance to the model. The reason is clear.

The model *Acræinæ* are of such extraordinary toughness and vitality that even should a bird follow up and peck at them, vital damage does not necessarily follow. The mimics, however, are much more brittle and non-resisting, so that if a bird, being in doubt as to their edibility, gives chase and attempts to taste and see, the butterfly might be seriously damaged.

The vitality of the *Acræinæ* is shown in many ways. It is difficult to kill them by pinching the thorax, and they will often resist for a long time the fumes of a cyanide bottle which accounts for other insects in a few minutes. The writer had noted this especially in the case of *Acræa mairèssi*, which did not succumb for more than half an hour in a powerful killing bottle; and some pupæ of *Planema consanguinea arenaria* twice survived an all-night sojourn in a cyanide bottle and were still kicking in the morning! The question of the appearance in life of models and mimics is so important when Mimicry is being discussed that one cannot resist dwelling upon it. In the case of *Pseudacræas* which mimic *Planemas* the writer was at first deceived over and over again by the appearance until he had learnt that the mimetic *Pseudacræas* rarely rest with the complete *abandon* of the *Planemas*. Moreover, if struck at and missed they dash away wildly and are gone for good, whereas one can alway reckon that a *Planema* will return to the spot within a few minutes if it has been alarmed, so callous do these protected insects seem.

Some of the mimetic *Lycænidae* (the family to which belong our "blues" and "coppers") have such an altered flight that the resemblance to the model is very greatly accentuated. The dancing, irregular flight of our common blue butterflies is as different as possible

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from the steady, straight, flight exhibited by many geometrid moths. Yet on the sandy foreshore of Bugalla Island the writer caught a Lycænid (a species of *Liptena* near to *L. kirbyi*) which was white with black border to the fore wings, and until he had got it in his net he thought it was merely a specimen of an extremely abundant and conspicuous Geometrid moth (*Geodena accra*) which it very closely resembles. The wild, dancing flight of a Lycænid had completely given way to a slow, steady, flight resembling that of the moth, which the butterfly so closely resembled in appearance, colour and pattern.

Or again, the writer several times sent to the Hope Department specimens of the Lycænid *Telipna nyanzæ* with the note that on the wing it was extremely like its model, a Geometrid moth of the genus *Aletis* in company with which it occurs. In this case, however, the resemblance was not so perfect as in the last.

But, sometimes the mimetic Lycænids still preserve the typical flight of the group to which they belong, although their resemblance to the models in details of pattern and colouring is very close indeed. Such is the case with *Mimacræa poulloni* whose flight is not like that of the *Acraea* which it mimics so extraordinarily closely.

One wonders, however, whether an enemy would notice and act upon the difference in flight of the mimic from the model, as does the collecting and presumably more intelligent entomologist.

The question of attitude and its importance as heightening the value of either aposematic (warning) or procryptic (concealing) colours is further illustrated by the following:—

A certain Arctiid moth (*Rhodogastria leucoptera*) was found resting fully exposed on low herbage. Its wings were of a pure, hard, shining white colour, but not very thickly scaled, so that when they were brought together over the body of the moth, the abdomen, which was of a bright rose pink, was distinctly visible; the thorax was pure white, spotted with black; the legs, which were freely displayed, were of the same bright rose as the abdomen. When the moth was disturbed it separated its wings and spread out the legs so as to display the bright pink (a typical aposeme), and emitted from the thorax just behind the head a copious yellow froth, till a mass of yellow bubbles with a very strong acrid odour (and, I may add, taste) projected on each side.

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Such frothing is a very common method of defence by aposematic insects. The abundant and very conspicuous Hypsid moth, *pactolicus*, gives out the same kind of froth when handled, and the writer has proved by offering it to moth-eating monkeys, that it is markedly distasteful, for they would never eat it.

A very beautiful example was again afforded by a large Acridian (grasshopper). It was a very heavy-bodied, slowly-moving species of dull leaden black colour with very small tegmina incompletely covering its small reddish wings ; its large and fat abdomen had red marks on the sides.

This insect is constantly to be seen crawling slowly and heavily over grassland and it is extremely conspicuous. It makes no attempt to get out of the way ; and only feebly hops an inch or so if much interfered with.

In short it has all the characteristics of a typically protected insect. In order to test whether it was really distasteful, the writer put one down in front of three young pet monkeys, who were constantly fed on grasshoppers, so that they would expect it to be good to eat, as they were accustomed to being given only edible species, and always became greatly excited when the box was produced.

In this case, however, instead of at once snatching it, biting its head off, and then devouring it piecemeal, one of the monkeys caught hold of it and looked at it with great interest ; it was obviously something which it would be better to examine first. The monkey was a young one who had been in captivity from babyhood, so that it was highly probable that he had never seen anything like this before.

While he held the grasshopper (which was of good size, being two or three inches long) it began to emit yellow, strongly smelling, acrid froth from the sides of the thorax, forcing it out by first distending the abdomen with air so as to show off the red markings on the sides, and then contracting the abdomen so strongly that the bubbles emerged from the thorax with a hissing sound audible several yards away. At the same time the red wings were prominently displayed.

The monkey was obviously greatly interested in this very curious phenomenon, and tasted the froth. He clearly did not like it, but

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as he could not believe that an insect given him by his master was not good to eat, he persisted in pulling it to pieces and tasting ; eventually the dismembered insect lay on the ground.

It was hardly possible to doubt from the monkey's behaviour that this conspicuous insect was highly distasteful, and that if he had been a wild monkey, able to select what food he would eat from out of a great abundance and had already met one of these markedly aposematic grasshoppers he would not think it worth while to try another.

The other two monkeys tasted and smelt at the remains, but would not eat them.

The effect of attitude alone in defence was very well exemplified by a large Saturnid moth which was put on the ground in front of these same monkeys. It was a large yellow species with well-marked eyelike spots on the wings. When alarmed it bent the body ventrally into a strong curve, and held the wings in a very curious and unusual fashion ; almost upright with the upper surface directed forwards so that the eyelike markings were extremely conspicuous ; indeed, the attitude was obviously intended to display these "eyes." The moth thus looked curiously weird and unmothlike, and the monkeys were afraid even to touch it.

It was not merely the size of which they were afraid, because they caught and readily devoured large and protectively coloured Sphingid moths often found concealed about the house.

Stress is here laid upon the value of attitude in the struggle for life, because those who strive to account for mimetic resemblances on the theory that they have been suddenly produced as a "Sport" by "Mutation," forget that the superficial appearance is by no means all that goes to make up a mimic, and that far more deeply seated changes are often necessary to perfect the resemblance (*vide supra*, the flight of mimetic *Lycaenidae*). When one begins to discuss the value of attitude in *concealment* the examples are so numerous that it is difficult to know where to begin or where to end.

The writer was very much struck with the case of a Noctuid moth of the genus *Cirphis* closely allied to our English "Wainscots." On the upper side the wings are light brown ; below, however, they are of a beautiful light silvery grey. The meaning of this is at once obvious when the moth is seen in its natural environment, where it

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adopts an attitude quite foreign to that of the majority of Noctuids. It hangs from a dry flower spike of tall grass with the wings brought together face to face over its back so that they hang down showing only the silvery underside, and the effect agrees extraordinarily well with the silvery grass head. The writer repeatedly saw it take up this attitude when it had been disturbed and had flown away to one grass head after another.

This brings out well the importance of seeing insects in their natural surroundings, for in this case a peculiarity in colouring is at once seen to be correlated with a marked departure from the attitude usually adopted by that particular group of moths.

A very wonderful example of procryptic resemblance brought out by attitude was afforded by a Notodontid moth which the writer found on a leaf on one of the islands in Lake Victoria (*Scalmicauda niveiplaga*). Only a single specimen of this species has been recorded hitherto, namely, the type in the British Museum. It had such a perfect resemblance to a dead and rolled-up leaf that the writer had to look again and again, and almost to touch it before he could satisfy himself that it was really a moth.

The fore wings, of a light-brown colour, were closely brought together along the back, hiding the hind wings, so that the two inner margins, of a slightly darker hue than the rest of the wing, came together along the middle line and represented the mid-rib of a leaf. The continuation of this into the leaf-stalk was represented by a large, upstanding, slightly curved tuft of long hairs, projecting from the top of the head.

The front of the head was very dark brown and represented exactly the dark shadow of the interior of a tube of rolled-up leaf. Strange though it may seem, this was the most realistic factor in the whole resemblance, and that which made it most difficult to realize that one was looking at a moth and not at a dead leaf.

The fore wings were light brown with several lines on them of a darker hue running out from the apparent mid-rib to represent veins on the leaf, and there were three doubly-ringed markings resembling the marks made by the growth of minute fungi on dead leaves. Near the tip was an absolutely pure white, small, round spot which quite well represented a gap at the edge of a dead leaf

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with strong light shining through. The antennæ and legs were so carefully packed away that they were quite invisible.

The writer thinks he has never been so completely puzzled by an insect resembling a dead leaf as by this moth, and yet when it had been set, and was in the Hope Museum, Professor Poulton was surprised to hear how much like a leaf it had been when alive.

One only realises to the full the reality of procryptic resemblance to surroundings when one has been absolutely and irrevocably deceived by it.

The perfection of the resemblance of Geometrid caterpillars to sticks is well known, but other and much larger larvæ are often quite as well concealed. The writer well remembers one occasion on Damba Island when one of his boys brought him a large caterpillar which he had found wandering on the bark of a tree. It was one of the *Lasiocampidæ*, the group to which belongs our English "Lappet," etc., and had the bark-like colouring possessed by the "Lappet" larva, which is very common among the *Lasiocampidæ*, namely, a mottling of browns and greys with innumerable fine dark streaks and lines.

The perfection of the resemblance to bark is very remarkable, and, as Professor Meldola first pointed out, is much heightened by the beautiful way in which the gap between the edge of the body of the larva and the surface on which it rests is hidden by the numerous small fleshy "lappets" ending in tufts of speckled hairs which project from the side.

It is very interesting to watch how, when one of these larvæ is walking and comes to rest on a suitable spot (usually a slight depression on the surface of the bark), it seems to flatten itself out and to spread out these little lappets with their tufts of hair along its side. When the boy brought the writer the above-mentioned specimen, he said there was another in a hole on the bark, so he went to look and marvelled at the perfection of the resemblance to the bark. Presently he became aware by degrees that what he had taken to be the bark at the edge of the hole was *another larva*. Soon, on another part of the tree they saw another, and while looking at it the writer gradually became aware that its immediate surroundings were not bark, but *more larvæ*, and in fact there was a group of *seven*, lying side by side and practically touching, the

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largest being in the centre and on each side three, in order of decreasing size outwards. The excitement and surprise of the boy was most amusing, as they gradually found that what had appeared to be bark was a *congeries* of caterpillars !

Insects that are procryptic at rest are, however, sometimes very conspicuous on the wing, but usually have then a very rapid and irregular flight. This is well exemplified by our "Red Underwing." They are often, as one would expect, extremely shy and difficult of approach and this the writer found to be particularly the case with a moth in Uganda, 2 fine large species of *Ophideres* with beautifully procryptic bark-like fore wings and bright orange hind wings.

The writer usually first saw this moth dart out in front of him in the forest, when its orange hind wings were extremely conspicuous.

It always made for the trunk of an adjacent tree and settled instantaneously head downwards, held its wings widely open for a brief second so as to display the orange hind wings, and then suddenly shut them with a snap and became almost invisible on the tree trunk, so well did the fore wings agree with the bark.

But directly he approached again it took to the wing, being extraordinarily wary and difficult to catch, and the same process was repeated as long as he could follow the moth.

The explanation of this would seem to be as follows :—

When at rest the moth is well concealed. Should an enemy approach too near it the moth takes to the wing, when the orange colour of the hind wings is probably, as the late Mr. J. Jenner Weir first suggested, of the nature of directive markings ; that is to say, a conspicuous part is displayed to attract the attention of the enemy ; but it is one whose damage or loss will not vitally affect the owner, so that if the bird pecks at it the insect gets away practically unharmed.

Having, however, eluded its pursuer, the moth again settles, displays the directive orange hind wings for a moment in case the pursuer is close behind it, then shuts its wings with a snap and becomes at once concealed by the perfection of its procryptic colouring from any new enemy approaching from a distance.

The value of such directive markings is very obvious to anyone who has worked in the field, and does not pay attention solely to "cabinet specimens," but catches and examines damaged specimens

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as well. Constantly, as Professor Poulton and Mr. G. A. K. Marshall have shown, one finds specimens which have been attacked just at the point where the directive mark had been : this had been bitten out and the butterfly or moth had escaped.

The long tails on the hind wings of certain "Blue" butterflies are extremely conspicuous and float behind the butterfly when it is on the wing in a very attractive manner, but they break off extremely easily.

It is argued by Professor Punnett that mimicry amongst Lepidoptera cannot have been produced by Natural Selection acting on small variations through the agency of birds, because, in his opinion, there is not sufficient evidence that birds do attack butterflies and moths.

Does he then consider mimicry amongst Lepidoptera to be on a different footing from mimicry in other orders, or better still, from Mimicry of one arthropod by another of a very different class ?

Some of the most marvellously deceptive cases of mimicry that are known are the resemblances of *spiders* to *ants*, where an arachnid with eight legs, without antennæ, and with head and thorax fused into a single mass resembles so closely an insect, with six legs, long antennæ, and head, thorax and abdomen all separated from each other by marked constrictions, that experienced field naturalists have been deceived over and over again. There cannot, surely, in this case, be any doubt about the agency of birds, who are well known to destroy spiders in large quantities. In these cases of mimicry of ants by spiders the importance of movements and attitude can again hardly be over-rated, since on them depend the perfection of the resemblance. What a very deep-seated change must have occurred to change the gait of a spider into the typical hurrying, rather uncertain, and agitated movements of an ant ! On June 26th, 1912, on Damba Island the writer was looking at a nest of the ant *Oecophylla smaragdina*. This ant, by means of silk spun by one of its own larvæ which it holds in its jaws, fastens leaves of trees together to make its nest, the resulting structure being approximately globular and of the size of two fists.

After he had been for some minutes looking at this he proceeded to box one or two ants for specimens, when to his great astonishment one of them suddenly *jumped* and disclosed itself as a jumping

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spider, subsequently identified as *Myrmarachne fœnissex*. He soon saw one or two others and was able to watch them running about among the ants, who took no notice of them. The front pair of legs was carried in the position of the antennæ of an ant, and they were moved quickly and restlessly in exactly the same manner. Moreover, when disturbed the spider, as it were, "came to attention" with the anterior legs held up in the air facing the source of disturbance exactly after the fashion of the antennæ of an ant.

The whole body of the spider was long and thin and there was a marked constriction in the abdomen to represent the "waist" of the ant; in colour and size ant and spider were alike.

These spiders were so at home with the ants that when alarmed they ran inside the nest, and on opening a nest the writer found inside it a spider which appeared to have freshly moulted, as it was soft and white.

Subsequently the writer found, on a nest of this same ant, a mimetic Capsid bug, of a genus hitherto unknown. The bug also ran about among the ants and moved its antennæ in an ant-like manner, which again implies a great alteration in the instincts, for the brisk movements of an ant are wholly unlike the deliberate and slow movements characteristic of Hemiptera as a whole.

This bug was of long and of unusually narrow shape—the abdomen was much constricted at the base, and the waist of the ant was represented in the bug, when seen from above, by a narrowing at the bases of the wing covers. In size the bug was slightly bigger than the ants, and though not quite such a wonderful mimic as the spider, it still had a very marked resemblance to the ant.

The parasitic Hymenoptera of the family *Braconidæ* are very often mimicked by other insects; they are typically "protected" insects with a disagreeable odour and are, when large, very conspicuous on the wing, and often of bright, aposematic colours. Certain large species are black with yellow heads and long antennæ, also yellow.

One day in June, 1912, on Bugalla Island, the writer saw what he took to be a very fine large Braconid on the wing, and gave chase.

To his great pleasure he found it was not a Braconid, but a Longicorn beetle, a species of *Dirphya*, beautifully mimetic. The Longicorn

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beetles, as a group, are rather long and narrow, so that a good starting point is provided from which a resemblance to the thin-bodied Braconids could be worked up by Natural Selection.

This *Dirphya*, however, was unusually narrow bodied, and the elytra were even narrower. The antennæ and head were bright yellow and extremely conspicuous when the beetle was flying.

The appearance of a waist, necessary to carry out the resemblance, was produced by the lower part of the base of the abdomen being as it were painted out ; it was of a curious glistening white colour, rendering that part of the insect invisible, so that the remaining black very well reproduced the appearance of the real waist of the model.

When this beetle was held between finger and thumb the deceptive resemblance to a Hymenopterous insect was still further carried out by the protrusion from the abdomen of a very flexible, bright yellow structure, which was moved about in a threatening way and strongly suggested the idea of a sting. At the same time the beetle produced the stridulating noise in the manner usual amongst Longicorns. This was by no means the only example of a Braconoid Longicorn which the writer met with, but as it was the first it naturally made the strongest impression upon him.

The reality of the struggle for existence forces itself upon the notice of anyone who studies the enemies of insects.

In the course of investigations into the bionomics of the Tsetse fly in Uganda the writer shot about 150 insectivorous birds with the object of discovering whether any had devoured the fly. Although one *knows* that birds *do* eat insects, yet one could never overcome a feeling of wonder at the enormous destruction which must take place, as evidenced by the extent to which the birds' stomachs were found tightly packed with insects.

A very interesting point is the fact that some birds make a speciality of certain insects ; sometimes those very species which we supporters of the mimetic theory claim to be specially protected and to act as models for other less well defended species.

This was pointed out, as an objection to the theory, by Professor Punnett in his article in BEDROCK, Vol. II., No. 2, where he argued that the Wood-swallow in Ceylon lives almost exclusively on the specially protected Danaine butterflies, so that in his opinion

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it would be a danger rather than a protection for other species to resemble them. At first sight this seems plausible enough ; but it is no argument against the protection of the Danaines from *insectivorous birds in general*.

The writer, for instance, has found in Uganda that two species of bee-eater (*Merops superciliosus* and *Melittophagus meridionalis*) prey almost exclusively on dragon-flies and the undoubtedly protected Hymenoptera. Honey-bees in particular were devoured ; and the African honey-bee has a very powerful sting, and is the model for a mimetic two-winged fly, a species of *Eristalis*, which copies it wonderfully closely.

The writer has on more than one occasion found as many as a dozen honey-bees in the stomach of one bee-eater ; and other species of Hymenoptera found in the stomachs of both were the "stink ant" *Paltothyreus tarsatus* (a winged male), a species of wasp of the genus *Belonogaster* which has a very venomous sting, and a large Braconid, of typical warning coloration, probably an outlying member of a large combination mimicking the Lycid beetles, described by Mr. G. A. K. Marshall.

On one occasion a *Melittophagus* was found to have devoured only male and female winged ants, it having been shot just at a time when the sexual forms had emerged in a great swarm ; and on another occasion a small flycatcher was found to have made a large meal from nothing but medium-sized "ichneumon-flies" (parasitic Hymenoptera).

The very hairy caterpillars known as "Woolly Bears" and those of the family *Lymantridæ* to which belong our "Gold tail" and "Vapourer," which have, besides numerous long hairs, thick tussocks of short and closely-set hairs, are certainly specially protected against birds in general. But our English cuckoo is known to prey almost entirely upon hairy caterpillars, and in Uganda the writer found that several cuckoos had been true to the tradition of the family, and a species of the closely allied coucals (*Centropidæ*) appeared to feed very largely upon hairy caterpillars, although not exclusively, for the writer has seen one with a butterfly in its mouth.

It is, of course, perfectly obvious that every species must have its enemies whether it is "protected" or otherwise, or else it would soon increase beyond all limits.

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The protection on which supporters of the mimetic theory lay so much stress is directed against *vertebrates in general*, and if the protected insect is not, as are hairy caterpillars, preyed upon by some specialised vertebrate foe, then it will be found to be destroyed in correspondingly greater numbers by other enemies, either by parasites during its egg, larva, or pupa stata, or by predaceous insects.

When one rears Lepidoptera to any large extent from eggs, larvæ, or pupæ, one is soon struck by the fact that any specially protected species is very largely destroyed by parasites.

The writer obtained in Uganda seventy full-grown larvæ, or pupæ, of *Acræa zetes*, a member of a typically protected family and itself conspicuous on the wing, being bright red and black. Of these seventy, however, no less than 77 *per cent.* were destroyed by parasites, there being among them two species of Hymenoptera belonging to the families *Braconidæ* and *Chalcididæ*, and a species of two-winged fly (*Tachinidæ*).

Now, inasmuch as these were all full-grown larvæ, or pupæ, when they were obtained, 77 *per cent.* probably does not represent the full tale of destruction. It is possible that the eggs are destroyed by Hymenopterous parasites, or the young larvæ by spiders and predaceous insects, as the writer has seen to be the case with another species of *Acræa*. So that, *at the most*, only 23 *per cent.* of the offspring reach the butterfly stage, which may also have its own enemies.

As Dr. A. R. Wallace long ago pointed out, of any two parents of any species, only two of the offspring can survive, on the average, to produce young in their turn.

So that, broadly speaking, in the case of *Acræa zetes*, if we put X for the total number of offspring, then $X - 2$ equals the destruction by all influences. Putting P for those destroyed by parasites, *p* for those succumbing to predaceous insects, and V for those eaten by vertebrates we have

$$V = (X - 2) - (P + p).$$

It is obvious that the larger is $P + p$, the smaller must be V, so that if one finds any species particularly subject to the attacks of parasites, as is *Acræa zetes*, one can predicate small destruction by vertebrate foes. Since the larval stage of Lepidoptera is one during

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which enormous destruction by vertebrates take place, we often find that the larva has become of very highly specialised form or colour.

Sometimes this specialisation is directed towards making the larva more conspicuous, in which case it is usually either highly distasteful or provided with hairs, spines, etc. But this protection against vertebrates is of no avail against parasites or predaceous insects or spiders.

The writer has seen the gregarious, and typically aposematic, larvæ of *Acræa perenna* being devoured by a predaceous bug (*Pentatomidæ*), and has found their skins, sucked dry, in the retreat of a hunting spider.

A brood of *Acræa terpsichore* was eaten up one night by the raiding ants *Dorylus* which found their way into the box through a crack, but pet monkeys would eat neither larvæ, pupæ, nor butterflies.

The writer reared large numbers of caterpillars on Bugalla and Damba Island, and was much struck by the degree to which certain very peculiar larvæ of moths of the family *Limacodidæ* were destroyed by parasites.

These larvæ were of very curious shapes and unlike any others ; they had the head entirely concealed beneath the anterior extremity, their legs invisible, their bodies often without any trace of segmentation and beset with rosettes of extremely hard and sharp spines.

Added to this they were often of extremely bright colours, yellow and green, and very conspicuous. An interesting point about them was that they were unable to feed in the manner usual amongst caterpillars, since the absence of definite legs prevented them placing themselves along the edge of the leaf, so that they rested on the flat surface, and bit out pieces in a plane at right angles to the edge.

It is hardly possible to doubt that they would be avoided by at any rate the majority of birds.

The writer found single larvæ of some half-dozen species, and three specimens of another, yet all of them save one were destroyed by parasites, and that particular one was the least well-protected of them all, having no spines and being of a quiet grey-green colour, so that one might expect that birds *would* eat it.

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These field notes will, it is hoped, provide further evidence that the explanation of certain types of colouring as "Protective" and "Warning" is not merely a fanciful invention, but that they are of real value in the struggle for existence.

It is a strong argument in favour of the truth of the mimetic theory that the resemblance of one insect to another is explicable in exactly the same way as the resemblance of an insect to a dead leaf.

In both cases protection is afforded by resemblance to a particular small portion of the environment which, in one case is a dead leaf, and in the other an insect. The fact that the latter is distasteful to birds seems a poor reason for putting Mimicry in a class by itself, to be explained by some special theory.

On the theory of Natural Selection, through minute variations, mimetic resemblances are simply a special case of coloration analogous to other special cases.

There are, of course, difficulties in the way of fully explaining everything by the theory of mimicry, but it is hoped that the examples given, which are only a selection from the writer's observations, will help to smooth away some of the objections raised against the great theories associated with the name of Bates and Müller, which owe their origin to the immortal Darwin.

LANGUAGE, ACTION, AND BELIEF

By J. Ceridfryn Thomas, B.Sc.
(Keridon)

MAN has devised nothing as yet for his weal which he has not been able more or less to pervert into a means of woe ; and so universal is this rule that even human speech is no exception to it. So superlatively “ divine ” was the acquisition that it enabled him to partake of the “ fruit of the tree of knowledge ” and become “ equal with God.” Nevertheless it has covered the earth with legions of the hideous spectres of untruths which are ever pursuing him as a pack of relentless furies.

I may for the purpose of this article classify the ultimate contents of mind as *ideas* and *beliefs*. These mental elements are materialised in language, the words of which form their discrete bodies. Within these verbal embodiments man has imprisoned, distorted, and tortured the “ ideal soul ” which in revenge has retaliated by making speech a fertile and a perennial source of falsity in every department of thought. But it is in connection with beliefs that language has inflicted upon humanity the full blight of its curse. Beliefs are our springs of conduct—our very guides in life—hence our weal and woe are bound up indissolubly with our opinions and our creed.

An idea of an object or event becomes a belief when it is fused or welded with the attribute of “ reality.” That is the essence of belief. But with the exception of one group, not much of this welding is done by the individual himself ; for language is the human market of “ ready-made ” beliefs which are communicated to each

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individual in the process of conscious or unconscious education. In this way each new generation gets the bulk of its ideas and beliefs from the preceding one. To a great extent they are imbibed along with speech, in which they are all more or less dissolved.

Beliefs are divisible into two classes—those belonging to this world of sense and phenomena, and those pertaining to the “spirit world.” But the first class again subdivides into two, so that we have in all three orders or groups of beliefs.

The first group includes what is generally known as “common experience.” The bulk of it refers to the manifold properties, relative to each other and to the human organism, possessed by the legion of common objects around us. In the case of these much, if not most, of the welding is effected within ourselves by our own sense-impressions and more especially through our sensations of pleasure and pain. These beliefs may be said to be coined in the mint of our daily experience; and what gives them the validity of “truths” is their uniformity and universality—that is, they affect all humanity alike, a fact which is specially connoted in the term “common experience.”

The next order of beliefs refers to the interpretation of Nature or the explanation of its phenomena; and in this factory it is reason that plies the soldering-iron. The verdicts given by its labours, however, do not attain the status of truths until repeated application of the process by different people and at different times gives substantially consistent results. Comparatively little of this welding, or belief-making, takes place within the average individual, for in no department has man adopted the principle of the division of labour more completely than in the industry of thinking. This class is generally referred to as Science.

The third order, however, differs *toto coelo* from the other two in having no legitimate relation to either experience or reason. They are implanted in each person as ready-made beliefs by authoritative and dogmatic testimony. Since their subject-matter lies wholly outside common experience, the canons of reason cannot be applied as tests of validity. Their sole foundation is testimony—the recipients of any one age becoming, by a kind of “alternation of generations,” the authority of the next. And yet for some time past, man has been troubled by serious misgivings that mere human

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testimony, however august, is not at all a satisfactory ground for accepting certain spiritualistic ideas as realities in the absence of experience and in opposition to reason. In this dilemma a portion of mankind has resorted to many devices with a view to retaining its grip upon these beliefs. The devices adopted for this purpose belong to two different and even diametrically opposed orders. The apologist either treats them as "supernatural revelations" which are wholly independent of the sources of human knowledge; or he passes over to the opposite camp and assumes that these also are related to experience and can be defended therefore by reason.

According to the first attitude, all the "knowledge" we have of the "spirit world" is claimed to be a "revelation" from that "world." This assumption was supposed to be effective in one of two ways: either by soothing reason with an opiate or by banishing it from court.

For it apparently secured higher credentials for these beliefs than was afforded them by the mere testimony of certain individuals. The notion, however, is a pure illusion; for only a moment's reflection is necessary to show that it immediately reduces itself to simple testimony. Let it be granted that a revelation *was* made to a certain person on some special occasion; but, whatever be its nature or contents, it was a "revelation" only to the one individual concerned. The rest of mankind had only his word for it. If any accepted it as truth, their belief was based *not on revelation* at all, but on the testimony of the certain individual. Their immediate object of belief was the veracity of the visionary and not the contents of the vision.

But probably the more effectual was the ousting method, for by claiming it to be a "revelation" it was hoped that the spiritualistic "verities" would be kept away from the corroding touch of impious doubt. For this assumption surrounded the objects of faith with such a halo of sanctity that they were considered too sacred to be touched with the dissecting knife of doubting reason. And as long as this kind of quarantine was effectively maintained, beliefs pertaining to the supernatural were safe from injury.

But despite all watchful care the demon of doubt crept in. A section of those on the watch towers abandoned the old safe policy of *credo quia impossibile*, and passed over from the attitude of

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infallibility to the fallible methods of the human intellect, and claimed that their faith was *rational* as well as "revealed." Consequently reason was requisitioned to underpin the tenets of faith. And for centuries torrents of arguments, evidences, and "proofs"—subtle, learned, and logical—poured forth incessantly from the high arcana of learning, to disappear as regularly in the sea of oblivion ; and Doubt meanwhile waxed bold and aggressive. This result was inevitable, for reason can no more uphold belief in "spirit entities" than a lever can exert uplifting force without a fulcrum, and from the same cause—the want of something solid to rest on.

The pendulum has at last swung right over to the other end ; for a certain section of the community (to a more detailed consideration of which we shall return in the second part of this article) claims that beliefs pertaining to the "spirit world" are based on foundations having the same nature and solidity as those of common knowledge and science, viz., that they rest on "experience" like all other knowledge, only that it is of the abnormal variety, such as insanity, epilepsy, double personality, trances, dreams, and hallucinations of all kinds. It is certainly a fact that these are true experiences ; for their manifestations are known, subjectively or objectively, to all mankind. It is not, however, upon these pathological *facts* that the spiritists build their castle, but upon their psychic *contents*, which unfortunately are devoid of every attribute that imparts to common experience its solidity as a basis of belief. Dreams are facts, but their contents are not. The mental contents of these sad phenomena are so completely outside ordinary experience that they cannot by any possible means be brought even to its bar for trial. They are individual and personal and wholly confined to the visionary himself. They may be intensely "real" to him, but that alone is worthless as evidence of their objective reality. The asseverations of a person suffering from delirium tremens, from the effects of a fever, or from insanity, have absolutely the same evidential value as those of a Swedenborg, a mystic, or a spiritualistic medium even when that person happens to be a "non-charlatan." The contents of hallucinations are comparable to—in fact, they are the analogues of—the poisonous products developed in the system during disease : the material system manufactures poisons ; the mental, visions.

How can a belief be then engendered in other minds when it has

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no basis in the facts of experience ? Two facts make it one of the easiest of human achievements.

In the first place it matters nothing that there exists no reality corresponding to the idea which is the basis of belief ; for that reality can be *made to exist in the language which communicates it*, by the simple device of coining terms and phrases which assume such existence. And these verbal embodiments have greater power of generating belief than even sense-impressions. A Roman youth found no difficulty in believing that Jupiter and Juno were real beings, for they were real beings in the language which conveyed to him the ideas. So dependent is belief upon speech that if a story is hall-marked by the age as " real " it matters nothing whether the event ever occurred or not, or even whether it be at all possible without arresting the whole course of Nature.

The other fact that makes the propagation of belief an easy matter is its own " infectious " nature. Belief is a contagion, an infection, a leaven which spreads itself if placed in a suitable medium. It is an electric charge which duplicates itself by induction. The only difficulty is at the start. If a visionary succeeds in getting some persons to accept his tale as genuine, his battle is practically won. The greater the number and the more famous they be, the more rapidly it will spread. After epileptic Mohammed made his " first converts," he found it comparatively plain sailing. All that the propagandist then needs is dogmatic assumption and the adoption of such language as will unmistakeably assume the reality and intensity of the belief of the converts.

For this purpose, speech has a most powerful ally in action. Nothing will convince people more readily of the existence of a genuine belief than acting on it. The highest confirmation one can offer of the existence of an alleged conviction is to direct one's conduct by it. Action or conduct is like the presence of a slab of paraffin between the plates of a condenser ; belief has then its maximum inductive power.

Let us now submit to a brief examination, in the light of the foregoing remarks, some of the noted features of the Association which styled itself " A Society for Psychical Research." But before I proceed to point out the resourcefulness of that society in the use of language I wish to draw attention to the effect that its foundation

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had upon general belief in spirits. The formation of the society together with its séances was "faith in action." It was the staging and the acting of the ghost doctrine. It was a cardinal event—a red-letter day—in the life-history of the spirits. It revived them like the blast from the four winds which breathed life into the dry bones that Ezekiel saw in the vision. The effect here was just as remarkable ; for the spook was in a state of suspended animation or indeed was on the point of being asphyxiated in the suffocating atmosphere of science. But by becoming the proud object of serious discussion it suddenly sprang into new life. To be discussed by dons of learning with genuinely serious faces was unto it the "savour of life unto life." As a result of this reawakening the spirits left their dim-lit haunts in myriads and even grew so bold as to frequent anew the regions of light and life ; they have often taken up their abodes amongst us, amusing some, terrifying others, and assisting others to live ; and here they will remain as long as hut and palace will offer them open doors and warm hospitality. In this spiritualistic reaction and revival, however, language, and not action, has played the premier part. We live in an age when language is strained to its uttermost to suggest and to engender belief in the untrue and the unreal. This sad fact is probably most in evidence in the advertising world of commerce, in which, alas ! it has become a recognised industry. But as an art, it has nowhere been pursued with more skill and diligence than by "The Society for Psychical Research." It has shown itself as fully alive to the paramount importance and magical aid to be derived from language as if it were a propaganda with a burning mission to proclaim and not, as it pretends to be, a society for "research."

Its nomenclature is a new coinage, bright from the mint, and specially struck off for the use of the society. It includes amongst others the terms : clairvoyance, clair-audience, astral-stage, psychic-force, telepathy, second-sight, levitation, automatic writing, re-materialisation, double-sight, super-normal, thought-reading and thought-transference—*every one of which is a question-begging epithet* ; that is, its function is simply and solely to insinuate beliefs in ideas and not to store or revive them.

Its literature moreover abounds with phrases and sentences ingeniously obscure. For exquisite vagueness they may be compared

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to the design of a pattern on a revolving disc where all melts away into an indefinite haze—the whole object apparently being to suggest the existence of something, but so indefinitely that no point is visible at which the arrows of logic and reason can be aimed.

But of all its belief-creating epithets and phrases, its own title of “psychical research” is *facile princeps*. The first word is intended to inform the public by cryptic suggestion, that psychical phenomena form its field of investigation, while the second word “research” is similarly intended to suggest that the methods of science are applied in this investigation.

Now to make my indictment perfectly clear, let me state at the outset that the application of the term “orthodoxy” to the prevailing current creed of any age or persuasion is incomparably less arrogantly assumptive than the phrase “psychical research” is to the proceedings of the society which appropriated the phrase as its distinctive title. The two words are palpable and unblushing examples of the *suggestio falsi*.

I know of nothing comparable to it save the word “science” in the title “Christian Science.”

Whoever invented the phrase “psychical research” was as skilled in the art of instilling or propagating faith as the notorious Mrs. Eddy. The more familiar equivalent of “psychical” is “mental”—an epithet signifying all those phenomena which make up the entire contents of mind and which form the data of the science known as psychology. By thus hoisting a standard bearing such a term one would naturally infer that its field of “research” corresponded more or less with that of psychology and that its “findings” or the results of its “research” would form a mental science!

This inference, however, is wholly false. In the first place the phenomena which it professes to have selected as its data of investigation are *not* the mass of actual and real phenomena which make up the contents of mind, but that very hypothetical class which is correctly denoted by the terms “occultism” and “magic.” Now if that sincerity of purpose which goes with the absence of all prepossessions characterised the aims of the society, it would have, without hesitation, adopted “occult” and “magical” instead of “psychical” as its qualifying epithet. They are both singularly free from ambiguity and connote exactly those “phenomena”

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which the society ostensibly proposed as the objective of its labours. The term "occult" accurately describes their obscure and elusive nature and "magical" their absolute independence and freedom from natural law. Despite this fact, it adopted a title which connotes neither obscurity, nor elusiveness, nor miracle. And the reason for it is too obvious to be mistaken. The terms "occult" and "magical" have come to connote in the public mind what is unreal and imaginary, and therefore tend to engender *disbelief* in the "phenomena" in which its members more or less believed—a belief, moreover, which they have been more than a little anxious that others should share.

But that is not the whole of the untruth suggested by the term "psychical." One would conclude that "research" was directed at least upon such "phenomena" as the term suggested, however inaccurately.

Nothing could be again further from the truth. The so-called phenomena are too elusive for examination at all—granting their reality. To make them the subject of "research" is too ludicrous for words; these "phenomena" are never to be caught—they elude one's grasp as a shadow and vanish like darkness before light. Research may cast its net far and wide, but these "phenomena" will pass through its meshes unseen. They participate to the fullest extent in the attributes of the ghost. They are never actual phenomena, but like the spook itself—only alleged and reported ones.

In truth, the subject-matter of the so-called research consists simply of diverse *allegations* of people in respect to supposed strange experiences.

In the case of séances, its avowed object is to test the truthfulness of the more or less neurotic performers. It was not the "phenomena" but the *honesty* of the "phenomena" that the society was supposed to have in view. Its professed object was *moral* and not "psychical."

The total absence, however, of any trace of a scientific spirit in its "proceedings" is made manifest by the fact that it has devoted none of its attention to an investigation, even in semblance, of those psychical phenomena which form the root and stem of truth upon which its mass of fungus growth has got fixed and fed, viz., the

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abnormal and pathological phenomena manifested in hypnosis, both natural and induced, in somnambulism, in epilepsy, in dreams, and in hallucinations of all kinds. These true and obscure phenomena have not been even approached in the scientific spirit, that is, with a view to understanding and elucidating them.

C'est tout le contraire. Instead of being a subject of research, those phenomena have been made *use of* as instrumental conditions or means of communicating with a "spirit world"! They have served the spiritualists only as a ledge of truth upon which to plant their pretensions and professions of spiritism.

It is to this implied falsity, so noted in all their assumptions, that Sir Ray Lankester directs attention in the January *BEDROCK*, when he exposes the suggested untruth in the specious phrase "The phenomena in our midst." And precisely the same indictment is preferred against the society by Sir Bryan Donkin throughout his trenchant article in the same number by his uniform application of inverted commas to every term of its nomenclature.

Having now disposed of the word "psychical" let us next direct our attention to that of "research." If it be possible the indictment against this term is stronger still. It unmistakeably suggests the application of the "scientific method" in the investigation of phenomena as when used in reference to physics, chemistry, or biology. It is not possible to conceive of conditions more opposed or antagonistic to those adopted in scientific pursuits than those which obtain in the *séance*.

Even nominally it possesses none of the characteristics of a scientific investigation. It is professedly more comparable to a gathering of police experts who come together to verify and check the professions of a Houdini than to the conducting of a scientific research. Practically every essential of the latter is conspicuous by its absence and by the presence of its opposite. With the view to bringing this fact into fuller relief, let us try and sketch the conditions that must obtain in the laboratory before a semblance of an analogy can be instituted between it and a *séance*. In the first place, every flask, retort, and piece of apparatus should be endowed with "consciousness and will," so that they could introduce the factor of volition into the process; next, they must often be endowed with powerful motives to exert their utmost to effect deception; further,

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the laboratory must be reduced to a state as unfavourable for accurate observation as is obtainable in practice—it must be night-time, and the gas jets must be lowered to the point of extinction.

And yet the parallel is not nearly complete, for the apparatus themselves have a word to say in the matter—they must be allowed to impose certain conditions or the experiment will refuse to come off ! Moreover a considerable amount of skill must be allowed to each flask to obscure or hide the conscious part it plays in bringing about the result. And lastly, the atmosphere of the laboratory must be pervaded with an aura of sweet amenity so as to make it impossible for critics to adopt a hostile attitude towards it.

What scientists in the wide world apply the term “ research ” to such a grotesque travesty of scientific experimentation ? and yet the term is unblushingly applied to the performance of a séance. Contrast mentally a séance with a psychological laboratory like that attached to an American university as a place for “ psychical research,” and you place a genuine coin by the side of a base counterfeit gilded to appear “ real and true ” by being dipped in the vat of speech. The fact is that the “ incarnates ” who have undertaken to champion the cause of the “ dis-incarnates ” have at last grown bold and arrogant, because the prosecution, for the sake of peace, urbanity and goodwill, had voluntarily left the court. Outside, possibly, they often screwed their faces, and shrugged their shoulders, but refrained from re-entering the “ lists ” until the very excesses of the advocates, in claiming the results as “ discoveries,” “ established facts,” etc., goaded Dr. Tuckett to enter a protest which the bold “ benchers ” of the “ Inn of the spirits ” resent bitterly as an impudent intrusion by an “ utter outsider.” The absence of any check for so long a time has filled the “ advocates ” with such overweening as to make them indignant at being pulled up at all—a very natural result ; but their resentment will not be misunderstood by readers of BEDROCK who have followed the interesting discussion which has appeared in its pages during the past twelvemonth.

DR. ARCHDALL REID ON RHETORIC

By Hugh S. Elliot

DR. ARCHDALL REID, in the last number of *BEDROCK*, asks me certain questions, which I hasten to answer. As he truly remarks, his criticisms tend to bear a hostile aspect. He will, therefore, forgive me if mine also bear an appearance of hostility. Let me begin, therefore, by assuring him of my great regard for his published books, and of the pleasure which I derived from their perusal. In general, it seems to me that we face the world from a very similar standpoint. Our opinions, if differing somewhat in species and even perhaps in genus, belong, nevertheless, to one family; and the present discussion is one of those domestic, fire-side disputes which (as everybody knows) turn upon very small differences, and which yet (as everybody also knows) are viewed from so close a range as to convey an untrue impression of magnitude.

I have read Dr. Reid's criticisms with care, and find no occasion whatever to alter my previous position. As Dr. Reid himself remarks: "The matter is largely one of definition—of precision of language." It is so; and the question is whether he or I have used language in the more correct sense. Of that, readers must judge for themselves. I pass to the questions asked by Dr. Reid.

"The point," he says, "concerning which I desire enlightenment is why Mr. Elliot should so dislike the incursions of metaphysicians into biology. Whether they be Idealists, Deists, Materialists or any other sort, they cease to be metaphysicians when they enter the domains of science; they do not discuss metaphysics."

I dislike the incursions of metaphysicians into biology for two reasons: (1) that they are not acquainted with the facts which are essential to a profitable study of that science; (2) that the method of metaphysics is radically unsuited, and, indeed, excessively

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dangerous, in such a science as biology. The biologist reaches truth by way of observation and experiment. He looks outwards into nature for the source of his knowledge. The metaphysician knows nothing either of observation or experiment; and very often holds them in contempt. His method is that of dialectics, and long chains of reasoning, which, even if they started from facts (as they rarely do), would in biology afford only the very slenderest chance of terminating in truth. The metaphysician does not look outwards into nature for his knowledge; he looks inwards into the bottomless depths of his own soul. Intuition is his guide. He sees in his dreams a writing on the wall, and immediately proclaims it as universal truth before which mere vulgar facts must ruthlessly be kicked out of the way. I object to the incursion of metaphysics into biology for the same reason that I object to the incursion of bulls into china-shops, and of carpenters into surgery; and I cannot agree with Dr. Reid that they cease to be metaphysicians when they enter the domains of science. Does a carpenter cease to be a carpenter and become a surgeon by merely attempting an operation for appendicitis? When he had opened the abdomen with a gimlet and a chisel, and was rummaging about inside the patient in a vain attempt to find the appendix, should I be incorrect if I were to say "there is a carpenter," and would Dr. Reid be justified in replying, "No, there is a surgeon"?

If it were necessary to give any further reasons for my dislike of the incursions of metaphysicians into biology, they would be afforded in abundance by the astonishing absurdity of the conclusions reached by metaphysicians when they make such incursions. But surely I need not labour this point. Let Dr. Reid open a modern work on spiritualistic metaphysics, and peruse it with a biological eye. He will, I think, find that the author hangs himself with much greater certainty and precision than I could ever hope to attain in doing it for him.

Dr. Reid's second point concerns my suggestion that Darwinism rested on a teleological basis. The suggestion is resented by Dr. Reid; but it surely must be obvious to everyone that he and I mean quite different things by "teleological." Dr. Reid defines it as "the argument from design in proof of the existence of God." I cannot bring myself to believe that Dr. Reid really thought I

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was imputing any such attribute to Darwinism. I was using the word in a totally different sense ; a sense which in recent years has to a great extent superseded the older definition. I used it, as the context clearly showed, to indicate that the idea of purpose lies at the basis of Darwinism. Natural Selection is teleological, because it subserves a purpose and achieves an end : viz., the end that the fittest survive. This has nothing to do with conscious or human purpose, as Dr. Reid suggests. It is in no sort of way derogatory to Darwinism. If structures are useful to the organism, as Darwinism assumes, then the structures are teleological ; and the theory which affirms that they are useful is a teleological theory. Dr. Reid writes : " Running all through Darwin's thinking is the assumption that the structures of living beings are useful to them." That is all I intended to suggest ; the assumption is teleological in the usual modern sense of that word, and the fact that it is so is no sort of reflection upon the theory itself. Let me give a few authorities in favour of this use of the word.

Lange, in his *Geschichte des Materialismus*, written soon after the publication of the *Origin of Species*, pointed out that Natural Selection (which he warmly accepted) was essentially teleological. Professor Sherrington, in his *Integrative Action of the Nervous System*, p. 235, writes : " In light of the Darwinian theory every reflex *must* be purposive. We here trench upon a kind of teleology." Professor Starling, in his *Principles of Human Physiology*, p. 5, writes that " adaptation " involves " the teleological conception that every normal activity must be good for the organism." Yet both these distinguished physiologists are as good Darwinians as Dr. Reid himself. If Dr. Reid will look up the article " teleology " in the *Encyclopædia Britannica*, he will find that these writers have used the word in no unusual meaning, but in the current modern sense of the word. Dr. Reid continues :—

" Neither Loeb, nor the Mendelians, nor the Mutationists have demonstrated, or even attempted to demonstrate, that Darwinism has a teleological basis."

But why should they ? In its more modern meaning, Darwinism is beyond all question teleological. That is exactly what " teleological " means. In its older meaning of conscious design, few people (and certainly not Loeb, Mendelians or Mutationists)

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would accuse Darwinism of teleology. Dr. McDougall, indeed, has done so in his *Body and Mind* ; and since I have devoted some space in BEDROCK to repelling this very suggestion, I find it difficult to understand why this attack should have been made upon me, instead of upon Dr. McDougall.

One further citation I must make from Dr. Reid. He writes that the statement that Darwinism has a teleological basis

“ has been, whenever made, no more than a mere statement, unsupported by evidence or any sort of proof, a disingenuous appeal to the prejudices of biologists, an item of rhetoric dishonestly intended to exalt one supposition by discrediting another, a product of what is perilously akin to charlatanism.”

Dr. Reid, being safely delivered of this healthy fulmination, names no special individual on whose miserable head it shall take effect. I am still uncertain whether I am intended to take it for myself, or whether Professor Loeb is chiefly implicated, or the Mendelians or the Mutationists, or Professor Sherrington, Starling and others who have referred to the teleology of Darwinism. It is a polyvalent curse, like that in the Jackdaw of Rheims, aimed against a certain guilty person or persons unknown. But, as in the case of that famous bird, I have grave suspicions whether anybody will feel one penny the worse. I venture to affirm that none of us are under any misconception as to the *facts* of Darwinism, but only as to the meaning of the word “teleology.”

Now, as I previously pointed out, there are some who deny the universal utility of the structures and functions of living things. They affirm that certain kinds of structures and reactions are neither of any use to the organism, nor ever have been of any use, nor are bound up by any necessary connection with other structures and reactions that are or have been useful. Clearly, if there are any such structures and functions, they must have been developed otherwise than by natural selection. As Dr. Reid says, Darwin did not attempt to prove the general utility of structures, and Dr. Reid adds, “ I think he must have had some idea that people of ordinary intelligence would perceive the fact.”

That is all very well ; but will Dr. Reid please note that the alleged fact is now denied by authorities of the highest competence ; and that these same authorities have produced a series of examples

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in support of their contention. The present *fracas* is due to the fact that I conceived it my duty as a reviewer of Loeb's *Mechanistic Conception of Life* to draw attention to the distinguished physiologist's attack upon certain aspects of Darwinism. I took the opportunity at the same time of mentioning other independent sources from which a similar attack had been directed; especially Brehm's *Thierleben*, a work little read in England, but of very wide influence in Germany.* Let me repeat that, personally, I have no opinion on the matter. I confess that the doctrine of the universal and necessary utility of structures (present, past or correlated) appears to me a proposition requiring proof; and not revealed by the light of nature. "People of ordinary intelligence" perceive so many incredible things, that their opinion can scarcely be taken as an adequate basis for Darwinism. No one denies that, in the main, structure is teleological, and therefore natural selection is applicable. But that is a different thing from affirming that all structure *must* necessarily and *à priori* be teleological; that no sort of structure or reaction can conceivably exist, save those that have some purposive connection. The case is simply one for examining the evidence. Do we or do we not find it to be so? Here are certain instances named, in which distinguished authors deny the possibility of any teleological colouring. There are now three courses open to Dr. Reid: he may either ignore them, or he may fulminate against them, or he may point out some error in their facts or arguments. But it is only the last alternative that can contribute towards a settlement of the question. In taking leave of this discussion, I must congratulate myself on the success with which I have drawn Dr. Reid's attention to the matter; for few authors are so well equipped as he is for undertaking a rejoinder to this new form of Darwinian heresy.

* While this paper was being printed, I have struck upon a new attack on the structural purposiveness assumed in Darwinism. The attack is entitled "Du déterminisme et de la finalité," in the *Revue des Idées* for April and June, 1913, by M. Georges Bohn, a very well-known French writer.

ON THE CONTROL OF VENEREAL DISEASE IN ENGLAND

By J. Ernest Lane, F.R.C.S., Senior Surgeon
London Lock Hospital

IN the April number of *BEDROCK*, appears an article by Dr. W. J. Barrett on the Suppression of Venereal Diseases, describing certain measures which have recently been adopted in the State of Victoria, Australia, with that praiseworthy object in view. At a discussion on Syphilis, held under the auspices of the Royal Society of Medicine last summer, Dr. Barrett gave an outline of the steps which had been taken in Victoria with a view of stamping out this disease, and his article in *BEDROCK* amplifies and explains them in more detail. I have read this article with the greatest interest, for the question of the mitigation and even the abolition of these diseases is one to which for many years past I have given particular attention. I was the representative of this country at the International Congress for Sanitary and Moral Prophylaxis, held in Brussels in 1902; in 1906 I brought before the London Medical Graduates' College and Polyclinic a communication on the Prophylaxis of Venereal Diseases; and in 1909 I read a paper before the Eugenics' Education Society on the same subject. I have had exceptional opportunities of observing the ravages of these maladies in this country, since for upwards of a quarter of a century I have been surgeon to the London Lock Hospital, the only special institution for the treatment of this class of disease.

The purpose of this communication is to institute a comparison between the sanitary regulations relating to venereal disease in the Mother Country, and those adopted by one of her flourishing Colonies.

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In the State of Victoria, the extent and the danger of venereal diseases was a subject to which the attention of the Government was first drawn in 1908, through the representations of the medical profession attending the Medical Congress in Melbourne, with the result that the Government at once realised the seriousness of the situation, and set to work to find some means of minimising the evil. In order to ascertain to what extent these diseases were prevalent it was ordained that they should be made compulsorily notifiable within a given area for a certain time. At the end of that period, it was found that 5,500 cases had been reported, of which number 3,167 were proved to be syphilitic by the Wassermann test. These figures were a convincing proof of the prevalence of disease, and consequently the Colonial Government decided to furnish and equip a ward for the treatment of people of any class, except prostitutes, who were found to be suffering from these diseases in contagious forms. The Government further arranged for the free application of the Wassermann test to 2,000 hospital cases a year, and to all other cases at a reduced rate of payment. Legislation was also proposed by which any person sentenced to imprisonment for any cause, should, if found to be suffering from contagious venereal disease, be detained until he was no longer a source of danger to others.

Such, in brief, are the measures adopted in Victoria, as described by Dr. Barrett. What the result of them will be remains to be seen.

I pass now to the consideration of the steps which have been taken in the Mother Country to contend with the evil. From time to time the attention of the public and of Parliament has been drawn to the prevalence of venereal diseases and the dangers consequent thereon to the community, and the subject may have attracted some ephemeral notice; but since the repeal of the Contagious Diseases Acts in 1886, no serious attempts have been made to legislate for the prevention of these most preventable diseases. The Contagious Diseases Acts were repealed, after having been in existence for twenty-two years, in response to the strenuous opposition of a large and influential section of the public to whom repressive measures such as these were utterly repulsive. Regulation, as it is called, is still in vogue in many parts of the Continent notwithstanding the strongly adverse opinion expressed by a large number

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of the leading members of the medical profession at the Brussels Congress of 1902, already alluded to. The strenuous opposition to State regulation of vice was founded partly on the injustice of the laws, and partly on their inefficiency and futility. They were unjust, since they aimed only at preventing women from spreading disease, whilst placing no such restriction on men. The periodical examination of any woman suspected of prostitution was a degrading process, and the herding together in a hospital of a number of women, some of them hardened and irreclaimable, and some only just embarked on a career of vice, was not calculated in any way to diminish the extent of prostitution, or to give the younger women any chance of reform. Upon the portals of the institutions to which these unfortunates were consigned might well be inscribed the words, "Abandon hope, all ye who enter here." Further, a considerable proportion of those who were gaining their means of existence from prostitution were able to evade the regulations, and to carry on their trade without let or hindrance. From the medical point of view, it is a very difficult matter to say that such and such a woman is incapable of conveying contagion; and in the interval between the periodical examinations she might easily be capable of acquiring a fresh infection.

The Government was approached in 1899, by the leading bodies of the medical profession, who suggested the appointment of a committee to enquire into the prevalence of venereal diseases in this country, and to take steps to check the spread of such diseases. The response was that public opinion was not sufficiently informed on the subject to justify any action being taken by the Government, and since then the matter has dropped as far as any legislation is concerned. It appears that the general public is as apathetic now on this subject as it was then. The department of the Government to which the public health is entrusted is the Local Government Board, which has in its employ a body of medical inspectors skilled in matters of sanitation. This department controls all acts affecting public health; drainage and sanitary matters generally; the registration of births, marriages and deaths, vaccination, etc. It has the power to issue directions and regulations for the speedy interment of the dead; for house to house visitations; for the dispensing of medicines; for guarding against the spread of disease; and for

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affording to persons afflicted by or threatened with epidemic, endemic or contagious diseases, such medical aid and such accommodation as may be required. This Governmental department, then, is perfectly competent to deal with the situation, and to turn its attention to the prevention of syphilis and other forms of venereal disease. On the outbreak of any epidemic, such as small-pox, typhoid fever, or diphtheria, the assistance of the Local Government Board is invoked, and active measures are at once taken to contend with the situation; but to cope with a disease such as syphilis, which is constantly present in our midst, no steps have ever been taken, though this disease is responsible for a greater mortality, and for a far greater amount of misery and distress than any of the epidemic diseases above-mentioned. The old-time *dictum*, "*Salus populi suprema lex*," is defined in a work I have before me as "the main end of every Government should be the well-being of the people, the establishment of order and security, and the diffusion of social happiness," a somewhat liberal translation of the four Latin words quoted above. As against this, might be quoted "*Tempora mutantur nos et mutamur in illis*"; for times are indeed changed since the health of the community was the supreme desire of the Government in power. It is now the votes of the community that command the greater consideration, and any legislation which had for its object the prophylaxis of venereal diseases would probably not only fail to command votes, but, on the other hand, might alienate the support of a section of the community. There are those who consider that syphilis is a just punishment for immorality, who would disregard the bodily ills associated with that disease, and would prefer ministration to the souls of those afflicted, disregarding or ignoring the fact that a large proportion of the sufferers from this disease have acquired it through no fault or moral lapse of their own.

The stamping out of tuberculosis appeals to public sentiment, but the stamping out of syphilis presents no such attractions, though of the two diseases it is just as great a menace to the public health, whilst it is certainly more amenable to prophylactic measures.

The futility of appealing to the Government for any legislation for the prevention of venereal diseases has by this time been thoroughly realised by the medical profession, and since the year 1899 no further representations have been made in that direction.

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In 1903, through the agency of the late Colonel Long, M.P., a committee was selected from among prominent members of the Church and of the medical profession for the purpose of enquiring into the effect on the public health of venereal disease, its treatment, and facilities for cure. Since the death of Colonel Long this committee appears to have been abandoned.

The Committee on Physical Deterioration, appointed in 1894, also recommended the appointment of a commission of enquiry into the prevalence and effects of syphilis, "having special regard to the possibility of making the disease notifiable, and to the adequacy of hospital accommodation for its treatment"; but this recommendation was neglected.

In March, 1912, the Eugenics' Education Society approached the Royal Society of Medicine, and suggested that the Society should urge on all large hospitals the advantages which would result from a record being kept in future of the number of cases of venereal diseases treated, in such form as to be readily available at the end of each year for statistical purposes. The Society was further urged to consider the question of hospital facilities, for the early treatment of venereal diseases, and to outline methods of reform. Enquiry was suggested as to whether the present methods of instruction of medical students with regard to these subjects was adequate, but it was decided not to approach the Government with regard to any legislative reforms, until further statistical information was available. A large committee of the Royal Society of Medicine was formed, which appointed a sub-committee from which a report may shortly be expected. Following on this the Royal Society of Medicine instituted a discussion on Syphilis in June last with special reference to : (a) its prevalence and intensity in the past and at the present day ; (b) its relation to public health, including congenital syphilis ; (c) the treatment of the disease.

As regards its prevalence, the general impression appeared to be that the disease was decreasing both in its prevalence and in its intensity, though no very trustworthy evidence was produced on these points. From the figures of the Registrar-General, it would appear that the mortality due to syphilis per million persons living was, in 1910, forty-six, whereas, in 1884, it was eighty-four. The number of deaths registered from hereditary syphilis of children

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under one, per 100,000 births, was, in 1910, 115, whereas in 1884 it was 191. The figures of the Army Medical Department show a similar decrease ; in 1910, the number of recruits refused on account of syphilis was 15 in 10,000, whereas in 1884 the number was 106 per 10,000. The number of admissions or re-admissions to hospital for venereal disease was, in 1910, 65 per 1,000, whilst in 1884 the proportion was 271 per 1,000.

As regards its intensity the suggestion was raised that owing to the prevalence of the disease for years and even centuries in a community, the virulence of the disease might be diminished owing to the gradual syphilisation of the whole race, and that the condition known as parasyphilis was relatively more prevalent than active syphilis. By parasyphilis is meant a group of morbid phenomena, consecutive to syphilis, such as locomotor ataxy, general paralysis of the insane, and certain forms of arterial degeneration, none of which conditions respond to the ordinary treatment for syphilis. It was further pointed out that a race previously free from syphilis, was more prone to suffer from a malignant type of the disease, affecting the skin, the bones and internal organs, as was demonstrated in the case of Uganda into which the disease was only recently introduced, and in which these parasyphilitic affections are comparatively rare. No very original suggestions were brought forward for measures by which the disease might be brought under better control, and when analysed they amount to little more than the views expressed by me more than ten years ago. These in brief were : (1) Systematic instruction ; (2) Efficient treatment.

The ignorance displayed by the general public on the subject of venereal disease is appalling. Young men are sent out to earn their living without any elementary ideas as to the existence of such a danger ; young women of the wage-earning classes are equally ignorant of the risks to which they are daily exposed on their way to or from their employment ; the majority of parents are too ignorant or too squeamish to mention to their children the existence of venereal disease, and when the question of matrimony crops up, they show their solicitude for their offspring by enquiring into the financial rather than into the hygienic advantages of the alliance. The duty of every parent, when the question of the marriage of one of their children arises, is not only to enquire into the family history

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of the individual who is desirous of being introduced into their family circle, but also to require a medical certificate stating that he or she is not suffering from any transmissible disease. Many men who have previously contracted syphilis, subsequently desire to marry; but how many of them consult their medical attendant as to their fitness for matrimony, and how many doctors insist upon having a blood test taken, before giving a confident assurance as to the complete restoration to health of their patient?

Of equal importance is the instruction to be given by the doctor to any patient who has contracted venereal disease. The treatment of the patient is of no less importance than that of conveying to him a full knowledge of the source of danger he may be to others, a procedure which is certainly not universally adopted either in hospital or in private practice. Above all, much more attention should be paid to the instruction of the medical student on this important subject. My experience as an examiner in surgery makes it clear that a large number of candidates for the surgical diploma are not competent to treat these diseases. To remedy this educational defect, I would suggest the institution of a special department for the treatment of venereal diseases in every general hospital under the supervision of one who is thoroughly competent to give instruction in these subjects. In no branch of medical science has the spirit of research been more actively displayed during the past ten years than it has in investigations into the pathology and the treatment of syphilis, investigations which have led to discoveries of the most vital importance. In 1905 Schaudin discovered the "*spirochaeta pallida*" the micro-organism of syphilis, the detection of which renders possible an early and positive diagnosis of syphilis, thus enabling the surgeon to commence the treatment at the earliest moment. In 1906 a method of testing the reaction of the blood, known as the Wassermann test, was introduced, by means of which the presence of the syphilitic poison in the system could be determined. This is a valuable guide to the duration of treatment, and to the decision whether a patient is cured of his disease or not. A few years later came a most important discovery in treatment, by Professor Ehrlich, of Frankfort, of a method by which large doses of arsenic could be introduced into the system by means of a compound known as "Salvarsan" or "606." Through the agency of this

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drug the course of the disease was considerably modified, and the rapid disappearance of the early manifestations of the disease was thereby effected.

As it is in the early stages that the disease is a source of the greatest danger, owing to its then highly contagious properties, it follows that any treatment by which these stages can be abbreviated must be of enormous advantage to the community. It is a matter of regret that these material advances are not sufficiently recognised in many quarters, and that research meets with but lukewarm encouragement. As a conspicuous instance of this, I would call attention to the attitude of the King's Hospital Fund towards research in these diseases. The authorities of the Male Lock Hospital, which is, as previously stated, the only special institution for the treatment of venereal diseases in the metropolis, have recently had occasion to rebuild their premises, and consequently had to apply for pecuniary assistance to the body which has control of the funds subscribed by the charitable public for the maintenance of the hospitals—King Edward's Hospital Fund. The plans for the rebuilding of this hospital had to be submitted to a committee of this Fund for their sanction and approval before any grant could be obtained, and they met with the following criticism :—

“ The plans provide for a pathological laboratory in the basement ; this appears to the Committee to be a questionable necessity in a small hospital. They are of opinion that the basement might with advantage be rearranged, so that some of the numerous cellars might either be let off and so prove a source of income, or be used for therapeutical purposes.”

Though this hospital is certainly a small one as far as cubic capacity is concerned, yet the amount of work it does is quite out of proportion to its size, as will be realised when I state that the total number of attendances in the out-patient department amounted to 34,894 in the year 1912. From these figures, it may be gathered that the opportunities for pathological research in these particular diseases are unrivalled in this country.

Since efficient treatment is one of the principal factors in diminishing both the amount and the severity of syphilis, every facility should be given to the unfortunate victims of the disease for obtaining such treatment, especially in the case of those whose circumstances in life are humble.

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Believing as I do in the enormous benefit to be obtained by the administration of "Salvarsan," every patient on first presenting himself at hospital for the treatment of his syphilis should be strongly advised to submit to injections of this preparation. But the drug is an expensive one, and cannot be used indiscriminately, and at the Lock Hospital a charge is made for these injections, of which about 800 are now administered in the course of the year. One hospital is not competent to treat all the cases of syphilis in the metropolis, so either greater facilities for treatment should be given by the general hospitals by the provision of a special department for venereal diseases, or special dispensaries should be opened in different parts of the metropolis where advice could be obtained from some thoroughly competent practitioner, where the most modern methods of treatment could be carried out, and where the Wassermann blood test could be applied, in each case gratuitously.

THE HEAD-MASTER OF ETON AND THE NEW MYSTICISM

THE following correspondence between The Hermit of Prague, Canon Lyttelton, and the Editor of BEDROCK, has taken place concerning the article under the above title which appeared in the issue of July.

HEADLAND HOTEL,
NEWQUAY,
July 31st, 1918.

To the Editor of BEDROCK.

DEAR SIR,—I have no desire to “reply” to Canon Lyttelton. What would be the use of arguing that everything I wrote was *à propos* (as I hold it was) or of declaring that the thought of being “jocose” had never crossed my mind (which it never did)? The public would only yawn!

The charge, however, of making “things worse by attributing to me opinions which were simply given as a digest of Miss Curtis’ own words” does, I admit, touch me nearly. It would have been such a scurvy trick on my part—if done intentionally. As a matter of fact, that was just the very thing I took especial trouble to avoid. That, however, is no guarantee that I succeeded, and, if I could now detect any passage (or quotation) in my article capable of being so construed, I should, of course, ask room in your next issue for the most contrite of apologies—but find such passage (or quotation) I absolutely cannot. You see I only made four quotations from his article: of the first I expressly said that they “are not Canon Lyttelton’s own words”; the second is given as indicative of “his own encomiastic attitude towards this New Mysticism”; the third is described as “some elucidatory observations of his own” as to the nature of the New Mysticism; while the fourth is introduced, and as I still believe, rightly introduced as “the following remarks by Canon Lyttelton anent the mental healing of a broken leg.”

A friend has suggested that the Canon may have been misled by my saying immediately after the third quotation “There is now sufficient material before us to justify the question: Which would have surprised our paterfamilias most—the theosophical tutor, or Eton’s head-master?” But, obviously, all I meant was that now the reader knew the nature of the New Mysticism (from Miss Curtis’ and the Canon’s own words) as

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well as his own encomiastic attitude towards it, he (the reader) was now in a position to estimate the degree of surprise (if any) all this had caused him.

Perhaps you may be able to discover in some way what it was I wrote (or quoted) that led Canon Lyttelton to formulate so serious a charge? And it may be that were he to re-peruse my article he would find his charge foundationless?

Yours faithfully,
THE HERMIT OF PRAGUE.

GRANGEGORMAN,
OVERSTRAND,
CROMER,
August 20th, 1918.

DEAR SIR,—Thanks for your letter and enclosures. The paragraph on which the Hermit of Prague animadverts is quoted on p. 143. I intended that to be a gist of the New Mysticism teaching, *not* my own opinions. But I see that I did not make this quite plain, and do not wish to impute any bad faith in the matter.

I am,
Yours faithfully,
E. LYTTELTON.

CALEDONIAN HOTEL,
INVERNESS,
August 31st, 1918.

To the Editor of BEDROCK.

DEAR SIR,—Many thanks for forwarding Canon Lyttelton's communication. And so the Canon still contends, albeit implicitly, that I did "make things worse by attributing to me opinions which were simply given as a digest of Miss Curtis' own words," though he admits having discovered, since those defamatory words were penned, that my offence was an excusable one—excusable, that is, in view of the ambiguity of his own language.

Now, if what Canon Lyttelton has had to say corresponded, even tolerably, with actuality, there would, I think, be nothing more to be said. But, alas! its non-correspondence with actuality is, as a matter of sober fact, positively heart-breaking. I am not saying this for rhetorical effect. It is the simple truth. But you can judge for yourself.

To begin with, strange as it may seem, I never "animadverted" on "the paragraph" that "is quoted on p. 143" at all. My *only* reference to this paragraph—after expressing a fear that "the reader's conception of the New Mysticism might still be somewhat lacking in definition—was to the effect that "the head-master of Eton has fortunately favoured

THE NEW MYSTICISM

us with some elucidatory observations of his own." This and nothing else. It turns out, therefore, that it was on the strength of this line and a half that the headmaster of Eton—for there is nothing else to which the word "animadvert" can possibly refer—incomprehensibly blind to the absolutely controlling force of the word "elucidatory," thought himself justified in charging me with making "things worse by attributing, etc."! By a parity of reasoning, to mention the fact that somebody had made some elucidatory observations on the subject of Mormonism would be regarded as equivalent to accusing him of being a polygamist!

But another curious fact is seen to emerge. The grounds on which Canon Lyttelton withdraws his charge of *mala fides* are demonstrably non-existent—are, indeed, the figment of his own imagination. In questioning the clarity of his own language, he is doing it the most grievous injustice. There was no ambiguity in it whatever, and, in consequence, I never misunderstood it for a moment. That his purpose was to afford his readers as complete an idea of the New Mysticism as he could, by the aid of quotations from Miss Curtis' own words, supplemented by sundry explanatory remarks of his own, was as clear as daylight. Had it not been so, I must have felt some hesitation in introducing "the paragraph . . . quoted on p. 143" with the descriptive words "some elucidatory observations of his own," as I did. But I felt none.

No, if Canon Lyttelton had grasped the obvious scheme of my article he could never have fallen into all these astounding errors. It was simplicity itself. My first object was to give striking examples, in the shape of extracts from Miss Curtis' and the Canon's own words, of the (to me) hopeless balderdash that has gone to the making of this so-called New Mysticism. My second was to demonstrate, by further extracts from the Canon's own words, his (to me) amazingly benevolent attitude towards this bizarre Yankee product. My third was to raise—implicitly, but obviously—the question whether, in view of this amazing attitude, it was not an imperial calamity that the most precious pack of adolescents in the British Empire should be under his control. Especially, too, did I seek to draw attention to the very real moral dangers involved in prescribing systematic "Meditation and Silence" for boys not out of their teens.

And yet, forsooth, Canon Lyttelton could find in all this "nothing to reply to,"* though he did discover "after careful reading" that "most of the article" was "intended to be jocose"!!†

Yours faithfully,

THE HERMIT OF PRAGUE.

* BEDROCK, Vol. II., p. 145.

† *Ibid.*

CURRENT RESEARCH NOTES

I.—TRANSPLANTATION OF ORGANS.

AMONG the discussions in the different sections of the International Medical Congress held in London last August, that which followed the demonstration of Dr. Max Borst of Munich on the grafting of normal tissues, was one of the most interesting from a biological point of view. His communication was illustrated by some marvellous lantern-slides, which showed earthworms with interchanged heads, butterflies grafted one on to the other, and tadpoles similar to those originally produced by Born who demonstrated that these larval forms can be grafted together at different points, or a given individual can be provided with double the number of limbs. It is well known that the investigations of Alexis Carrel and his colleagues have shown that it is possible, if strict precautions are taken to avoid bacterial contamination, to transplant entire organs from one individual to another, and Guthrie has succeeded in exchanging the sex-glands of black and white Leghorn hens, and showed that this modified the colour of the chicks subsequently hatched from the eggs laid after the operation.

Among the specimens in the museum formed by John Hunter, who devised many of the most remarkable experiments in grafting, are those of a human tooth grafted on to the comb of a cock, and of the growing bones of the rabbit implanted under the skin of a doe. Although the transplanted tissues, in the cases just given, from one species to another, undoubtedly survive for a certain length of time, sooner or later they die and take no part in the existence of the animal on which the grafts are made. Were this otherwise, it would be difficult to understand those differences which enable one species to be separated from another, and, apart from the conditions favourable or unfavourable to the success of transplantation of tissues from one species to another, the more highly developed the individual, the less the chances of survival of the grafts. In cases where grafts are made from species to species, the results are on the whole more successful, but even in these cases the experiments do not bear a close examination. Many cases of skin-grafting are recorded, for example, the skin of the negro on the white race and *vice versa*; moreover, the cornea of the eye has been transplanted from one individual to another with success. Even in these cases the characteristics of the graft become lost, and there is a gradual substitution of the tissue-cells of the host for that of the transplanted material. In the case of cancer transplantation in mice, which has been so developed by Bashford and others, similar difficulties are

CURRENT RESEARCH NOTES

met with; transplanted cancer cells will not flourish in normal mice, and a cancer growth is most easily transplanted from one region to another in the same animal.

The comparative ease with which grafting is possible among plants when contrasted with the difficulty in animals would seem to show that the cells of animals, and especially the higher animals, are extremely specialised. A fragment of a begonia leaf will reproduce the entire plant, and the same is true for a few cells of some ferns. Cells which exhibit this behaviour must possess some of that formative plasma which in other cases, indeed in all animals, is apparently concentrated in the sex-cells which alone can reproduce the individual.

The views advanced by Max Borst, and supported by his experimental work, give no expectation that grafts either from one species to another, or from one individual to another, would survive for long. In order that any transplanted cells should endure it is necessary that the cells which are removed should possess the same characteristics as the host. What these may be it is difficult to say, but they must be inherent features of the protoplasm of each individual cell. Such bio-chemical characteristics functionally separate the cells, not only of species from species, but of individual from individual, of one sex from the other. These differences, however, diminish with the degree of blood-relationship, and are least in such cases where there is similarity of species and race, of age and blood-relationship. Therefore any question of successful transplantation in the higher animals has the best prospect of success when this is carried out on individuals most closely related one to the other.

II.—THE METHOD OF SERUM DIAGNOSIS OF ABDERHALDEN.

IN this country very little information exists with reference to a remarkable diagnostic method which was introduced by Emil Abderhalden of Halle, about two years ago. Though highly technical in its details, the method is one of great biological interest.

He has shown that the blood of women about to become mothers differs from the blood of other women. This acquires distinctive bio-chemical characters, although the general composition of the blood shows little or no change. Abderhalden has demonstrated that a specific ferment comparable in some respects to the group of ferments which play a part in the normal processes of digestion, appears in the blood and the recognition of this can be used as a means of definite diagnosis in doubtful cases. Rubsamen has made use of this test in 100 cases without a single failure, and Freund and Brahm have recently communicated no less than 160 cases where Abderhalden's reaction has enabled a positive conclusion to be drawn. It is not the purpose of this note to describe the method in detail, nor to do more than

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indicate that those who have made use of the reaction in cases difficult of diagnosis are satisfied that it is one which is almost infallible. Apart from the profound changes which are associated with reproduction in the human female, the fact that the blood itself possesses characteristic biochemical features which can be demonstrated with 1·5 c.c. of blood-serum is one of considerable biological significance.

G. A. BUCKMASTER.

REVIEW

THE RATIONALE OF PUNISHMENT, by **HEINRICH OPPENHEIMER**,
D.Lit., LL.D., M.D. (London : University of London Press,
Ltd., 1913.)

This book is the author's approved thesis for the degree of Doctor of Literature in the University of London, and is worthy to rank among the best works on the subject with which it deals. The earlier part consists mainly of an historical enquiry into the origin of the punishment of individuals by society, while in the latter many and various theories of punishment are described and discussed. Students of "penology" and "criminology" will find all that they want or will profit them in Dr. Oppenheimer's clear and interesting account of the history of punishment, and much to think about in his wise comments.

The last chapter contains an excellent summary of the prominent doctrines of modern criminologists, and a contrast is drawn between the two extreme schools, which are often described respectively as "biological" and "sociological." The author refrains from stating his own views explicitly, but, in referring to what he terms "criminological eclecticism," which apparently is a kind of synthesis of the above-mentioned theories, he quotes as follows from Enrico Ferri, whom he regards as its chief exponent :—

"Every crime is the result of the simultaneous and indivisible concurrence of the biological conditions of the criminal and of the social conditions of the environment in which he has been born, in which he lives and acts. The most fruitful measure of defence against crime which society can adopt is of a two-fold character, and both parts must be employed and developed simultaneously. On the one hand, the improvement of the social conditions as the natural prevention of crime ; on the other hand, permanent or temporary means of elimination according as to whether the influence of the biological conditions in the causation of the crime is almost absolute, or greater, or smaller and more or less curable."

Of this nebulous or vacuous pronouncement Dr. Oppenheimer makes the following admirable criticism, which, in our opinion, errs only in its somewhat excessive leniency :—

"Where the occurrence of an event depends on the interaction of two forces, surely it is impossible to apportion the shares which they have in the production of the phenomenon. The concluding sentence quoted from Ferri is about as scientific as would be the assertion that

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hydrogen is the more important element in the composition of water because two atoms of it, as against one of oxygen, enter into the formation of a molecule. And if the same author states that '*the biological factor of crime* is something specific which, so far, we are unable to determine, but without which all the other conditions, physical and social, are insufficient to account for all forms of crime and for crime itself,' we discover that one arm of the bifurcate criminological theory ends in a very big note of interrogation, whilst the other, as we have already found, points to a distant Utopia."

This final criticism apparently supplies an indication of the author's own opinions concerning crime and punishment, with which, if we read him aright, we are in entire accord.

H. B. D.

CORRESPONDENCE

To the Editor of BEDROCK.

NURSERY METAPHYSICS.

SIR,—At the end of an article on “Modern Materialism” in the April number of *BEDROCK* (Vol. II., No. 1, p. 41), Dr. McDougall suggests a visit to the nursery, where we

“may hear propounded by the fresh voice of childhood, some of the old riddles which the mechanistic scheme leaves as insoluble as ever. ‘Where does space come to an end? When did time begin? What was there before the world began? Why can’t I stop thinking?’”

And in his reply, entitled “Scientific Materialism” in the July number of *BEDROCK* (Vol. II., No. 2, p. 193), Mr. Hugh Elliot writes:—

“Surely the adult philosopher is far more overwhelmed at the thought of these ultimate mysteries, than any child can ever be. For he knows, as the child does not know, that they are for ever insoluble and beyond the range of the human mind; he suspects that, not only is there no possibility of comprehension of them by the human mind, but that there is no comprehension of them anywhere.”

Having had nursery experience of these very questions, I venture to submit, and in brief, answers to some of them, for which we are indebted to John Locke, whose *Essay on the Human Understanding*, written over two centuries ago, is not read so much as it deserves to be.

“Space” would come to an end where matter, in some form, did not exist; for space is really the measure of the size of objects, of the room they occupy, and of their displacements—empty space being nothing at all. If space had neither bounds nor contents, it could not affect our senses; and knowledge of it would be impossible, not merely because we should be ignorant of it, but for the reason that there would be nothing to know. Locke puts it thus:—

“It is not necessary to prove the real existence of a Vacuum, but the idea of it . . . and it would be as absurd to demand whether there were Space without Body as whether there were Space without Space, or Body without Body, since they are but different names of the same idea.”

“Time” began when living beings had become “brainy” enough to get, from their observation of moving objects, the idea of duration; for, if nothing moved we could know nothing of what we mean by

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"time," as there is no such *thing* as Time—though we can talk about it until we believe in its existence.

"Why can't I stop thinking?" Mosso proved that thinking could be stopped by anæmia of the brain; unconscious persons do not think much, and dead people cannot think at all; but, as long as we live and remain sensitive to stimuli, we are sensible of their effects, we think. *Sentio ergo scio* is probably more correct than Descartes' *Cogito ergo sum*.

Before the World began! What do we mean by "World"? Is it the Earth, the Solar System, or the whole of what we vaguely speak of as the Universe? The conception of a nebular origin of all these things is as far back as the human mind can reach; for there is no evidence of anything else. We have no experience of the forming of the original stuff, and speculation on its formation would merely lead to a further question as to how the Creator was created. Man is superior to most animals, because of speech and imagination; but by the former he often deceives his fellows, and by the latter he sometimes deceives himself—Mystics and Metaphysicians being apparently bent on mis-taking words for things.

SESAMY.

August 18th, 1913.

September 9th, 1913.

To the Editor of BEDROCK.

SIR,—I confess I have never given much study to the question of the boundaries of space, as that problem has always appeared to me to be beyond the boundaries of my intellect: nor am I now convinced by Sesamy's interesting quotation from Locke, to the effect that space is meaningless except in so far as it is filled by matter. Supposing it were physically possible to create an absolute vacuum inside a glass vessel, that is to say, to withdraw all the air and every other particle of matter contained in it, there would still remain the same space within the vessel as there was before the contained matter was removed. You have removed the matter, but surely (*pace* Locke) you have not removed the space—that would indeed be pouring out the child with the bath. In the same way, I can, without difficulty, imagine a boundary of matter or of ether, but I cannot imagine a corresponding boundary of space: for whatever point of view we may fix as marking such a boundary, I should always be able to indicate another point a yard farther off.

But here, alas, I reach the boundaries of the space allotted to me for my reply to Sesamy, though not of the matter which I had hoped to comprise in it.

HUGH S. ELLIOT.

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MSS. intended for the April issue should be sent in not later than February 20th.



Provisional Contents of the April Issue.

(Vol. III., No. 1.)

The April issue will include amongst other Articles

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SIR OLIVER LODGE, INTOLERANT, INFALLIBLE.

By Professor H. E. Armstrong, F.R.S.

Argument is generally waste of time and trouble. It is better to present one's opinion and leave it to stick or no, as it may happen. If sound, it will probably in the end stick and the sticking is the main thing.

SAMUEL BUTLER.

I HAD the temerity, some few months ago, to review Professor Soddy's fascinating book, *The Interpretation of Radium*, and quoted, in the opening sentence of the article, the apt expression "Radium, what crimes are committed in thy name!" which I had met with in a *Times* dramatic criticism. In penning this aphoristic sentence, the critic probably had little inkling that he was coining a concise phrase of singular meaning which may well become classic. It is not merely, as I remarked, that an entirely fictitious value is being put upon the substance—no doubt to the great benefit of a few interested persons—but the poor public, as always, is being gulled in every possible way and led to believe that Radium has magic curative properties which make its application desirable in spite of the extravagant expenditure this involves. It were time that those who so loudly proclaim its virtues justified the faith that is in them and gave proof that they are competent disinterested observers of the things to which they testify. Meanwhile, the proper scientific study of the most wonderful material the world has ever learnt to know is perforce vastly delayed—perhaps to the great detriment of progress—for who can say what mysteries may not be unveiled by the continued profound investigation of radioactive change. In any case, that those who are affected with the dread disease cancer should be so specially singled

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out as the victims of what often seems suspiciously near to quackery, when not the real article, is more than deplorable.

Passing from the sublime to the ridiculous, to consider a minor crime committed in the name of Radium, here am I the victim of a most uncompromising and thoroughly premeditated attack by so majestic a person as the President of the British Association. Why? Simply because I have had the temerity to put forward a carefully considered scientific explanation of the mysterious suicidal behaviour which is characteristic of this reputed element.

My article was published early in July. On July 31st my peace of mind was rudely disturbed by the sudden appearance of the following notice of motion :—

PROFESSOR ARMSTRONG AND ATOMIC CONSTITUTION.

In the April number of the quarterly journal called *Science Progress* appears an article signed H. E. A., in which that distinguished chemist at length accepts, though not without hesitation and sustained scepticism, some of the results deduced by physicists from the phenomena of radioactivity; but he takes the opportunity of restating and reinforcing his opinion that the inert gases—helium, for instance—are not really monatomic; an opinion expressed by Professor Armstrong soon after the discovery of argon.

To maintain this rather strained position in face of experimental facts, a considerable amount of what seems to me gratuitous hypothesis is required; and since it is desirable to come to a better understanding of this matter, I propose to criticise his attitude, in a friendly way, in the October number of the same journal.

OLIVER J. LODGE.

Sir Oliver thus beat the drum in advance, to announce his intention of correcting me, in the weekly journal called *Nature*. Naturally, I shivered in my shoes throughout August and September; indeed I was so conscious of coming annihilation that I did not think it worth while to take a prolonged holiday but used most of my time attempting, as far as possible, to arrange my worldly affairs.

It has so happened that in this interval I derived unexpected support from reading Jeffery Farnol's breezy romance *The Broad Highway*, wherein are described a couple of very serious mills in which Peter, the leading character, is perforce engaged: though very severely mauled in both encounters, he manages to carry out

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his colours with honour, gaining Circe, otherwise Charmian, through the first and, in the second, by knocking him down, helping his antagonist—Black George—to return to a rational frame of mind, so that eventually the two become reconciled, each settling down happily with the “Schatz” of his heart. May such be the result of my meeting Sir Oliver in combat !

The attack he has made upon me is in his very best pontifical style. I must confess, however, that I am much astonished at the degree of intolerance he displays and have some difficulty in recognising the “friendly way”: no doubt, it is that of the fond Puritan parent chastising his offspring for the good of his “mortal soul.” I feel, of course, that if I had any proper sense of the respect due to authority, I ought to regard myself as crushed out of existence, deserving only decent burial and to be forgotten; unfortunately, I come of an irreverent stock and as the Midland Sage has not only delivered his gage at me but has followed up his threat of punishment by hitting out very hard in the October number of *Science Progress*, I should show myself a poltroon were I to decline battle.

He has height, weight, pen and position in his favour but, none the less, I shall endeavour to reach above his belt and trust that my lighter weight may enable me to get in a blow occasionally which will not be without effect; naturally, however, I have no hope of being able to knock the “Big Black George” with whom I am confronted altogether out of time, as my prototype of the novel did.

Let me first protest against the statement in Sir Oliver’s letter that, in my incriminated article, I “at length accept, though not without hesitation and sustained scepticism, some of the results deduced by physicists from the phenomena of radioactivity.” I defy my august critic to prove hesitation, let alone sustained scepticism, in my words; as a matter of fact, I accepted the findings of the physicists without the faintest comment. All that I have done, beyond offering an explanation of the behaviour of Radium, is to suggest the correction of certain statements made by Professor Soddy and to urge precision in nomenclature. My critic cannot have read my article with any degree of care before writing his letter to *Nature*; for some occult reason, he ran off on a false scent from the outset.

My crimes, according to Sir Oliver, are several and serious.

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Horribile dictu, I have said things "obstructive to progress." I have transgressed the limits of conservatism by ignoring facts and wildly manufacturing hypotheses in order to sustain an old and superseded, exclusive and negative generalisation—mark the exclusive and negative generalisation; this last apparently is a thing which is anathema in Sir Oliver's mind, though what exactly he has in view I am by no means sure; apparently it is his short for denial but I am not clear what it is that I have denied. In view of the Birmingham address, I fancy too I may have sinned by proclaiming views which are decidedly atomistic—with little savour of continuity in them.

At the moment, my distinguished adversary is in a position which leads the public to regard him as the mouthpiece of our scientific fraternity. It is therefore of some little consequence that he prove himself to be a master of sustained logical argument and fair in statement.

Passing to his article, the first conundrum to consider is, What is an element? as apparently my conservatism is made manifest by my asking, "When is an element not an element?" and replying, "When it is Radium!" The underlying issue is one of no slight importance. I am one of those who do not believe that language is given us to disguise our thoughts—though I cannot but admit that it usually does and that it is often convenient for those who discuss mysterious subjects to attach vague or multiple meanings to their words.

I do contend, however, that Sir Oliver has no right to chide me because I have ventured to urge that we now have difficulty in defining an element precisely and have said that "Science is only compatible with correct etymology—it is the duty of science to be correct in word as in deed," an opinion to which I had given utterance in consequence of Professor Soddy's statement that the question, "How can an element or the atom of an element change?" has given rise to many arguments of etymological rather than scientific importance."

Unfortunately, perhaps, words hit me; my earliest introduction into the art of thinking came from reading Trench's *Study of Words*: his way of discussing the origin and meaning of words fell upon me, when a lad, as a revelation of method and was the

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foundation of the critical and heuristic proclivities through which I have so often got into hot water. If I may be allowed to say so, after reading much that Sir Oliver has written, I cannot help feeling that he has slight respect for words—in fact, I have somewhere read that he is prone so to use terms that “meaning anything, they may mean everything.” I regret to say, it is a recognised fault of our scientific fraternity that, of late years, we have often been careless in our use of words and that we have acquired the habit of coining a new term for every vague idea that enters our minds: with unconscious irony too, the age being Greekless, these terms are mostly derived from the Greek. To get over the difficulty, I have myself proposed that the Royal Society should publish a key to the terminological inexactitudes which disfigure the field of scientific literature at the present day and make it impossible for the worker in any one field to read with understanding what those in other fields have written or even what his colleagues write.*

I know probably as well as Sir Oliver does—Trench taught me so in my boyish days—that significance changes and that the meaning associated with original derivation is liable to be departed from gradually. But this is due largely to carelessness and either because original derivation is disregarded or because it is not understood and the true meaning of a word is often but vaguely present to the person who uses it. As the philosopher of *Weissnichtwo* tells us, there is metaphor behind all our significant words. If the metaphor be not patent, the word has little meaning to the user:

* While engaged in correcting this article, I have received proofs of a series of communications to the Faraday Society dealing with the Passivity of Metals. The subject is obscure enough in itself: it is made more so in these essays by the far too frequent and gratuitous use of a dubious and often stilted phraseology. Take a sentence such as the following, for example:—

“It was found that when the anode was slowly polarized, the potential gradually became nobler and then there was a rapid change to the passive condition.”

To understand what is here implied (by the term “became nobler”) and how it is that “noble” is used at all in such a connexion, the reader must know a great deal. It is to be supposed the writer wishes it to be understood that the potential rose; if so, why does he not say so and at once make it clear to his readers what he desires them to know? Until we get rid of all such pretentious and obscure writing, science will have no footing in our ordinary life.

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every classical scholar can put a meaning upon the word "oxygen," for example ; but there are very few who can put any depth of meaning into it, simply because they know nothing about acids.

My point is that the tendency has been growing of late years among chemists, *though unfortunately not among physicists*, to give precise meanings to the terms "element," "atom" and "molecule."

In my article I deprecated the return to carelessness. An element is defined in the dictionary as a fundamental or ultimate part. Say what Professor Soddy, Sir Oliver Lodge or any other member of the host of physicists may, to the chemist who endeavours to use and think in language of precision the elements are the ultimate parts of the materials which he is called upon to study, How otherwise is he to define an element ? Sir Oliver Lodge implies that an element is merely something which has a place in Mendeléeff's Series—but if we ask, what is Mendeléeff's Series ? echo can only reply, "The series of elements." We are thus condemned to the usual reasoning in a circle. Not a few compounds have definite spectra and compound radicles are well known which exist either as such or in combination with others—so these characters, to which Sir Oliver refers as criteria, are not distinctive of elements.

In like manner, it has been more and more the custom among chemists, of late years, to restrict the term "atom" to the ultimate particle and not to apply it to compound materials : the kinetic unit is always spoken of as the molecule by those who give thought to their words.

If the molecule of the reputed "element" radium be resolvable into sundry sub-radiums, α , β , γ , etc., also into at least half-a-dozen molecules of helium and an unknown number of the minutely material, mystical particles called electrons—then indeed, being so many things, an "element" may well be anything and it is difficult to define one in reasonable terms. If physicists are right, we shall, I suppose, be called upon ultimately to worship the electron as the one and only element or protyl.

Sir Oliver Lodge apparently is an advocate of obscurantism in diction : as a matter of practical politics—from the point of view of those members of the priesthood of science who desire to be credited with oracular attributes—there may be something in it ; but to my mind such a policy is absolutely unscientific.

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In closing this section, I can only say that I remain uncontrite and of the opinion expressed in the article in *Science Progress* though often previously : for example, in my address delivered at the meeting of the British Association at Winnipeg in 1909 (*q.v.*). Also I may remind Sir Oliver of Lord Kelvin's words—

“The ‘disintegration of the radium atom’ is wantonly nonsensical. It is nonsense very misleading and mystifying to the general public, because, if what is at present called radium can be broken into parts, it is not an atom.”

Passing from philology to physics and chemistry, I have to deal with Sir Oliver's objection to my having given rein to my speculative instincts in the endeavour to account for the mysterious behaviour of Radium—I have to meet his charge “of quixotic tilting at ascertained facts the bearing of which I fail to understand.”

Surely, I may give him the *tu quoque* retort. It is clear that my monitor has failed entirely to understand my chemical argument. In years gone by, we have often been on the tilting ground together, more often than not as allies ; and among physicists, he has always seemed to me to be the most sympathetic and appreciative ; but now, apparently, he has lost touch with us. I have not tilted at facts ; I have tilted at an interpretation forced upon a fact.

“Radium,” he says, “is truly not a chemical compound but its atoms appear to embody a physical grouping such that definite substances result when it subdivides.”

If this be not obscurantism, I do not know what the term implies. And did he not tell us at Birmingham that “In science an appeal to occult qualities must be illegitimate.” He goes on to say :—

“This might be speculation, were it not that the emission of observed substances from radium actually occurs. In no chemical decomposition are atoms shot out with one-tenth of the velocity of light. The energy displayed is of a different order from chemical energy. In the effort which he makes to liken this kind of volcanic disruption to chemical decomposition, on the analogy of nitrogen chloride, Professor Armstrong is forced into hypotheses for which there is no basis whatever beyond his own speculative instinct.”

Surely whenever a “compound” is decomposed it is actually converted into “observed substances.” As to the energy displayed being

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of a different order from chemical energy—who has defined or can define or place a limit upon the “order of chemical energy”?—who shall say that when an effect is observed that is 10 or 100 or 1,000 or many thousand times as big as any previously observed effect, it is therefore of a different order? Are the dynamos of to-day of a different order from the puny machines of Faraday's time? We know that in principle they are not. As to my being forced into hypotheses for which there is no basis—I scorn the soft impeachment; had he gone back to his books and relearned a little chemistry, Sir Oliver would not have dared to bring such a charge; and may I remind him that, according to his own statement, “to deny effectively needs much more knowledge than to assert.”

But to consider my quixotic tilting at ascertained facts—what are the facts and what are they worth? what does Sir Oliver's argument amount to? He thinks it very wrong that I attach no particular importance to the $5/3$ ratio of the two elasticities (specific heats) in the case of Helium and the other Argonides and do not regard this as proof of the monatomic character of their molecules. I have from the outset declined to take this argument seriously, at first perhaps through “instinct” on general grounds; latterly, since the discovery was made of the decomposition of radium into helium and other substances, it has appeared to me to be practically inadmissible.

I can have no possible objection to the determination of specific heat by the acoustic method and why Sir Oliver should speak of my regarding the experiment as *despicable*, I am at a loss to understand: if he desired to be vituperative, he might well have selected expressions more appropriate and within the range of truth.

Assuming it to be based upon a sound generalisation, the $5/3$ ratio is merely proof that no energy is wasted in doing internal work within the molecules. But this is precisely what my hypothesis requires, my postulate being simply that the constituent “atoms” of the Helium molecule are so firmly interlocked that they behave as one—hence the all but complete indifference of the Helium molecule to external appeals. This interpretation, I suppose, is what Sir Oliver refers to as an old and superseded negative generalisation to sustain which I have wildly manufactured hypotheses. Wherein then am I a quixotic tilter against ascertained

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facts? I have not doubted any physical fact but have merely put a perfectly legitimate interpretation upon one—an interpretation which I contend is in accordance with the facts as we know them, especially the fact that the energy set free is of so high an order. I am free to confess that I have exercised my speculative instinct and even rejoice in the fact: Sir Oliver surely should not gibe at any one on this account. And I am comforted by a remark made by Faraday in one of his letters to Schönbein:—

“You can hardly imagine how I am struggling to exert my poetical ideals now for the discovery of analogies and remote figures respecting the earth, sun and all sorts of things—for I think that that is the true way (corrected by judgment) to work out a discovery.”

Why should not speculative instinct count largely? Instinct, Intuition, Imagination—call it what one may—is probably of vastly more importance than is commonly supposed even in the best circles. We were told, however, in the Birmingham Address that Science is systematised and *metrical* knowledge and that in regions where measurement cannot be applied, it has small scope. Mr. Balfour was quoted as saying—“Science depends on measurement and things not measureable are therefore excluded or tend to be excluded from its attention.” Following this came the statement from the President that scientific men may rightly neglect emotion and intuition and instinct in order to do their proper work but philosophers cannot. That Mr. Balfour should pronounce such an opinion was only natural, as the occasion called for it. He was speaking as the express advocate of an institution wherein the worship of remote decimals reigns supreme and imagination accordingly plays little part.

But these are the fictions that pass muster among too many physicists who have so long wandered in the fields of small decimals and so whittled down their intelligence to terms of the minus tenth that they have lost all sense of proportion and in particular have no biologic health left in them.

What are scientific men but philosophers and what are philosophers but scientific men? Did not Faraday love to call himself a philosopher and did not his imagination carry him far ahead of mathematical measurement? In point of fact, imagination both

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precedes and follows all measurements—measurement comes eventually but as the controlling agent and largely as the means of making conclusions comprehensible by minor intellects. The architect designs but translates his conceptions into figures in order that smaller men may carry out his ideas.

The miserable sterility of many of our modern workers is due to lack of imaginative power—too often they measure they know not what and when they have measured they can place no rational interpretation upon their measurements: but they go on measuring, as it is the one recognised proof of scientific respectability and the chief passport into the inner ring. Almost anybody can measure—few see what are the right things to measure and the right ways of measuring. In chemistry, absolute measurements are rarely possible apart from atomic weights and similar “constants.” Chemical properties, as a rule, are not singular criteria but the expression of reciprocal relationships which vary with the conditions and are not to be expressed in any absolute manner; unless we give rein to our fancy, we are nowhere. In the field of biology too, measurements play a subordinate part. This is by way of the defence of the use of “instinct” whether by scientific men or by philosophers, who, after all, are of one and the same class. Nothing of true knowledge, no method which will lead to true knowledge, can be excluded of or by science. In this sense, ours is the only way of exploring the depths of the universe. But having said so much against measurements in the narrow sense—physical measurements—I am free to confess that all things must be submitted ultimately to mental measurement, to logical analysis: this probably is where I should part company with Sir Oliver, as I see no measureable evidence, in this sense, of some of the things he contends for.

I believe my real offence consists in having given a chemical explanation of the behaviour of Radium—an explanation which is in complete accordance, I venture to think, with the accepted canons of chemistry and rational, not—*pace*, Sir Oliver—a wildly manufactured hypothesis. Apparently he now regards the study of radioactivity as exclusively the province of physicists; we inferior beings, the chemists, have no right to pronounce opinion upon it.

What Sir Oliver means, however, by saying of Radium “that it

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is not truly a chemical compound but its atoms appear to embody a physical grouping such that definite substances result when it subdivides," I am at a loss to understand. I can attach no physical meaning to his oracular statement that its atoms appear to embody a physical grouping. Such mysticism is beyond my comprehension. But clearly it puts us in possession of an original definition of an element.

I can assure Sir Oliver that though the specific heat ratio argument has long counted for little in my mind, the discoveries in connexion with Radium are neither "unwelcome nor indigestible" but the contrary. The chemical behaviour of radium—its spontaneous decomposition into helium and other substances—fills me with wonder and my scientific digestion is vastly improved in consequence; but it impresses me most as proof incontrovertible of the compound nature of Helium. On this view also, it is possible to understand that when shot forth from a radioactive substance, Helium has but a limited range: that is to say, after it has travelled a certain distance, it ceases to be a charged particle, though, of course, it continues its wanderings. It appears to me that the interval during which it parts with its double charge is possibly that required for the completion of the synthetic process wherein the "proto-helium" particles become associated and form helium molecules.

To conclude—Sir Oliver originally reproached me with having taken up a strained position in face of experimental facts and asserted that a considerable amount of gratuitous hypothesis is required to maintain my position. It appears to me that he has in no way justified his accusation but has merely put up a bogey of his own imagination and set to work to demolish it: if this give him pleasure, I can have no objection. None the less I am glad of and thank him for his criticism. Apart from the fact that it may serve to give my explanation—which I honestly believe to be worth consideration—some measure of bold advertisement, I hold it to be very desirable that we should dispute freely about these things. The path of science is becoming deadly dull in these days of detail and dogmatism, when each specialist airs his favourite doctrine but either resents criticism or is advisedly deaf to it. I have made complaint on these grounds during years past, especially against the exponents of the dogma of ionic dissociation—and Sir Oliver

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Lodge has been almost my only sympathetic listener and even my supporter, at times. Opinions stick, in these days, before they are proved to be sound—if uttered by those in authority. At all costs this must be prevented if science is to be of service to the State. Authority must be kept in order.

On this account, if space permitted, I should like to carry the war into my opponent's camp and to criticise some of the statements and conclusions in his recently delivered discontinuous Presidential address on continuity, as the position which he has taken up is in many respects one that is open to challenge and one that must be challenged sooner or later—but the task is not to be undertaken lightly.

MATERIALISM AND TELEPATHY

By the Hermit of Prague

I.

WHAT knowledge had we at birth of the existence of Space ? As far as can be inferred, none. And yet, before we were two years old, we knew, practically speaking, just as much as we did at twenty. What was it that we learned during those first twenty months ? And how did we learn it ?

Leaving out of account all tactual, visual, and aural impressions, which had as yet acquired no *meaning*, and ignoring all nascent conative stirrings, with the pleasurable and painful qualia occasioning, sustaining, and extinguishing them, our psychic stock-in-trade at birth was simplicity itself. It consisted of nothing but our three-dimensional feelings, or, as we should now describe them *the feelings in the body*. If the reader will tightly clench his right fist, and then introspectively observe the feeling in it, he will have a vivid illustration of what is meant by a three-dimensional feeling. That the fist and the feeling are *co-extensive* can be verified at once by exploration with the other hand. The fist and feeling, too, are commobile. Whither the one is moved, thither the other must go likewise.

The three-dimensional feelings in one's body are found, on introspection, to form a continuum ; that is to say, there is a continuity of feeling connecting each with each—connecting, for instance, the feeling in the head with that in the little toe, without a break occurring anywhere.* And this continuity is never broken,

* It would seem that three-dimensional feeling, or, it may be, the power of introspectively perceiving it, varies considerably. Everyone, however, would appear to belong to one of two types. Let the reader place the

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no matter how the relations between the different parts of the continuum vary (bodily movements).

Our bodies at birth were utterly unknown to us. This hardly needs saying. In fact nothing three-dimensional was known to us then, except our own three-dimensional feelings. And yet, as said before, our knowledge of three-dimensional space had practically matured before we were two years old. The immediate problem before us is how to fill that gap.

If our personal recollections preserved, in minutest detail, all our experiences during those first twenty crucial months, there would be no problem to be solved. But not only is this not the case, but, as a matter of fact, not one single individual memory of that most crowded period in our psychic history ever survives. The only course open, therefore, is to attempt an imaginative reconstruction. Such reconstruction, however, cannot possibly be made to correspond, as far as the order of the happenings is concerned, to actual life. In actual life, movements meeting with resistance—whether from another part of the baby's own body, or something other than its body—and movements meeting with none, occur in just whatever order chance decides. But in an imaginative reconstruction, it is indispensable, if we are to arrive at any intelligible result, that each of these types of happenings should be separately considered, and treated as if occurring in well-defined succession. We have, in other words, to make believe that, in our babyhood, first one type of movement was indefinitely repeated, and then a second, and so on. That movements which were spontaneous in the baby should now have to be made voluntarily, cannot, of course, be helped. With all this understood, our reconstructive experiments may be proceeded with at once.

Let anyone, *with eyes closed*, and standing with nothing within reach, make random movements with his limbs, focussing his

feeling in one hand next that in the back of his head, and that in the other hand next that in his forehead. Does he find, on introspection, a *plenum* of feeling (in the head) between the feelings in the two hands, or not? Some people unhesitatingly answer this question in the affirmative, while others describe the head-feeling as distinctly *peripheral*. If Professor Münsterberg would experimentally determine to what innate or acquired traits this singular difference is to be attributed, the results, most probably, would prove curiously illuminating.

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attention the while on the three-dimensional feelings within them and their (the feelings') interrelational changes. When this task has become familiar, let choice be made of such activities as lead to any two parts of the body being in contact, or, to express the same thing subjectively, to the feelings in the two parts in question becoming *next* each other. For instance, let such changes be made as end in the feeling in one clenched fist being next the feeling in the middle of the forehead, and let them be repeated an indefinite number of times. (The baby is doing this sort of thing every waking hour, for days and weeks and months.) Then effect similar changes with respect to the feeling in the other fist, and then with that in each knee (most people can bring the forehead and the knees in contact). When a number of similar experiments have been repeated and repeated, there emerges in time—doubtless through the functioning of memory and anticipation (inverted memory)—a sense of contrast between *nextness of feeling* that is actual, and nextness of feeling that it has been actual and may be so again. In other words, the permanent possibility—to use Mill's phrase—of a three-dimensional feeling having another three-dimensional feeling next it, has gradually achieved recognition.

In the experiments just described the different sets of changes never clashed. The second fist never approached the forehead till the first had been removed. Let us now take a step forward. With the left fist already in the centre of the forehead, let our experimenter initiate those changes that hitherto have ended in the right fist being similarly placed. What happens? The wonted changes fail to complete themselves. The feeling in the right fist, in lieu of becoming next the feeling in the forehead as before, now ends in being in contiguity with the feeling in the other fist instead.* Repeat the experiment, the fists exchanging rôles, and then vary with the knees. The result is just the same. Moreover, introspection unmistakably reveals the feeling in the left fist *between* the other two. If this experiment and others like it be indefinitely repeated, the discovery is eventually made of the permanent impossibility of two three-dimensional feelings being simultaneously next a third,

* The psychology of pressure and arrested movement does not concern us here.

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and when this discovery is coupled with the one previously made as to the permanent possibility of any three-dimensional feeling having another next it, the idea inevitably shapes itself of the three-dimensional *room* a feeling occupies, as distinguished from the particular feeling occupying it. There have come into being the fundamental and complementary conceptions of *room filled* and *room empty*. But, it must not be forgotten, the sole thing yet known to our experimenter as capable of filling room is his own three-dimensional feeling. And now a further step forward becomes possible.

Let our experimenter lie on his back, place something, say a small flat piece of wood, on his forehead, and then once more proceed to bring the feeling in his right fist next the feeling in his forehead. He cannot. Something *absolutely new* in his experience has happened. There is no three-dimensional feeling of *his own* next the feeling in his forehead, *and yet the room next it is as much filled as if there had been*. The experiment can be varied in endless different ways—with unvarying results. How is this novelty to be accounted for ?

There is only one answer for a mind so furnished as our experimenter's. As a filler of room the only thing he wots of is his own three-dimensional feeling. What fills the room in question, therefore, *must* be three-dimensional feeling *other than his own*. At first sight this may appear too big an inferential leap ; but when one recollects that the three-dimensional feelings in his fists and knees, etc., *have always been "other" to each other* since the experiments began, there is nothing left to marvel at. But lo, and behold, our experimenter *has now discovered matter* : matter, at this stage of the experiments, being, for him, simply three-dimensional feeling not his own but like his own.

Our experimenter, however, has another discovery awaiting him—the discovery of his own body. To save time, let us make this as easy for him as we can. He having placed the feeling in his right fist next the feeling in his right knee, let us anæsthetise the knee by an injection of cocaine. Again, what happens ? The three-dimensional feeling in the knee ceases to exist, *and yet the room next the feeling in the right fist remains filled*. It is not that the feeling in the knee has moved into other room. Nothing of the kind. It has

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simply ceased to be. And yet, in spite of its annihilation, *the room is not empty*. What fills it? Alas! our experimenter is at the end of his tether. That his own feelings could have been changed into feelings not his own is unthinkable. It is filled, therefore, neither by his own three-dimensional feeling, nor by three-dimensional feeling other than his own: and his memory, as was said before, has nothing else to suggest. He is, in truth, face to face with an absolutely insoluble problem. Nay, worse; the whole of his psychology—which has all been put together, it must be remembered, since the experiments began—has been reduced to chaos. If his own three-dimensional feelings do not fill room, then that hitherto vastly illuminating inference as to the existence of other three-dimensional feelings like his own but not his own, is robbed of all conceivable validity. Matter has become for him now simply filled room: what fills the room being utterly beyond conjecture. At this point, our experimenter might be mentally so overwhelmed as to even feel sceptical whether room is ever really filled by anything at all, and be inclined to think, in consequence, that “impenetrable room” would be a more appropriate term. If so, we should prescribe another (and a last) experiment. Let him place the impenetrable room that goes by the name of his right fist in contact with that known as his left temple, and then, without breaking contact, pass the former across the forehead. He would then have certain impenetrable room (his cocainable fist) moving through the room next his forehead. Room moving through room: *quod est absurdum!*

After this last experiment, we may imagine, our experimenter's conception of matter would be best expressed by some such phrase as room- or space-blackage.* Nevertheless, whenever he wished to think of such space-blockage, there would still be no other thought-material available wherewith to do so, save that which he had hitherto employed. And if he eventually experimented, *with his eyes open*, long enough to enable vision to acquire its true anticipatory meaning, anticipatory, that is, as regards what movements on his part were externally possible and what were

* The preceding argument is not, perhaps, altogether unlike the operation known as kicking a dead horse. For, surely, the idea of space being impenetrable without anything to render it so, is quite unentertainable.

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not, he would find, in practice, that the *qualitative* way he represented the space-blockage about him was a matter of indifference, since the avoidance of *undesired* collisions—the only thing that mattered—entirely depended on how it was *quantitatively* thought of: the quantitateness of both space and space-blockage being measurable by him, *primarily*, only in terms of his own three-dimensional feelings. For instance, the size of one's clenched fist can be measured introspectively in terms of the co-extensive feeling within it; which feeling, in turn, can be quantitatively compared—again by introspection—to that in the little finger of the other hand, and so on.

Subsequently some physicist might demonstrate to him that the space-blockage he had thus become acquainted with was, in reality, nothing but an aggregate of those tiniest known space-blockers commonly called electrons, and, if he lived, he might be shown by yet another physicist that these electrons were merely aggregates of tinier space-blockers still, and so on, until ultimate physical analysis is perforce abandoned. But this would throw no particle of light on what the last discovered tiny something really was.

In fact, when one reflects, it becomes self-evident that no conscious being can *know* what the something that holds or blocks space really is, unless that something should happen to be similar, that is *qualitatively* similar, to one of the conscious being's own three-dimensional states of consciousness. And even if it were, how is any conscious being to find it out?

It is often said by metaphysicians that to talk of knowing what a thing is *in itself* is meaningless. And yet if you were to tell the man in the street that he does not know what a brick is in itself, he would regard you as no better than an idiot. Why? Firstly, because what their feelings are in themselves is known to everybody. They cannot help it. Take, as examples, toothache and the feeling in the clenched fist. Secondly, because the inferences, made and *stereotyped* in babyhood, as to the various material objects about us consisting of three-dimensional feelings similar to, but other than, our own, remain, as a rule, unquestioned and, indeed, unrecognised, as long as life lasts. Moreover, even the few philosophers that do become aware of their unwarrantable existence are as little able to dispense with them, where conduct is concerned, as the man in

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the street himself. In fact, these basic inferences simply have to take their places among all the other life-preserving illusions—such as the independent objectivity of sound and colour—which Natural Selection has so skilfully bestowed upon us. That animals share them with us it is impossible to doubt. But as to whether the psychic germs from which they sprang existed in any of our uni-cellular ancestors, who can say ?

Fully assured, then, as the great majority of our fellow-citizens are, that they really do know what matter is, and being able to measure the space it holds in terms of their own feelings, it is not to be wondered at that, when they speak of it, they should employ such contemptuous terms as “mere” and “brute” and “blind,” or, when referring to Materialism, such epithets as “debasing” and “degrading.” But when it is fully comprehended that what the indestructible something that holds space is, must ever remain beyond every conscious being’s ken (save under the condition given above), such language will automatically cease. And if some seekers after truth believe that the one ultimate entity behind consciousness is the absolutely unknowable something that holds space, what better name could they have chosen for themselves than that of Materialists ? *

II.

Most searchers after truth, however, do not regard matter as the ultimate entity behind consciousness. There are other entities, they believe, in far more intimate relationship therewith, whose existence is conditional on that of matter in no way whatsoever. These entities, admittedly, are spatially unexplorable. Their existence can be proved, therefore, only *indirectly*. This is obvious, for the only things introspection can reveal directly are states of consciousness.

Now one of the entities so postulated, if the word may be forgiven, is Mind. Mind for the materialist (who quite understands that the only mind he can have immediate knowledge of is his own) is but the permanent possibility of thinking, which, he contends, is absolutely dependent on, and wholly accounted for by, the

* Matter, in its general or philosophical sense, embraces everything implied by such words as motion, cohesion, etc.

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existence of a brain. Thought, therefore, according to him, can only be communicated from one person (or brain) to another through the material organs of sensation. Consequently, were it to be incontestably established that thought can be transferred in any other way—I speak as a materialist—materialism would, *ipso facto*, be irretrievably discredited.

If this, as it assuredly would be to us, amazing thing *did* happen, we should, of course, admit its logical implications without any further ado. There would be no attempt to “save our faces” by skulking behind the preposterous “brain-wave” theory; nor should we raise the plea that the entity thus attested *might* still prove a product of matter after all: a plea that could be supported by no argument whatever, save that most ignominious argument of all, that the negative is incapable of proof. No, if this (to us) new entity did confront us, we should do, I trust, the only thing worth doing, and help, as best we could, to find out all about it.

But, it will be said, has not the Psychical Research Society been, to all intents and purposes, proving the existence of mind as an immaterial entity ever since 1882, and by this very method? This is a question that hardly admits of a direct answer. That a number of distinguished men of science and other earnest students, whose ingenuousness is wholly beyond suspicion, do believe that thought transference has been adequately proved, cannot be disputed. But here, for all practical purposes, the matter ends. All the efforts that have been made to convince the world in general that this belief of theirs is justified have virtually failed. Nor is it hard to find the explanation. The major reason is the admitted fact * that experimental success *is not to be relied on*. For this signifies that the very procedure that would carry conviction most swiftly and most surely, such as a startling exhibition of thought transference before a committee of the Royal Society, *cannot be arranged for*. For if it were attempted, it might, and if nervous agitation really can, as alleged, inhibit the telepathic faculty altogether, probably would, end in a humiliating failure. This, however, in

* Sir Oliver Lodge, *BEDROCK*, Vol. II., No. 2, p. 58: “But whether incipiently widespread or not, and however it be explained, the faculty of telepathic receptivity certainly exists in a few people; though even in them *it is by no means always and under all circumstances available*.” (Italics ours.)

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no way proves that the gentlemen referred to have been mistaken. It indicates only that the settling of the question must depend, not on dramatic demonstrations, but on the patient application of the law of averages.

But, again, is not this precisely what the S.P.R. have been doing all along ? And, if so, why have not their results been universally accepted ? This is due, to speak quite plainly, to the uncertainty that has always existed as to how far those results were to be relied on. No one, needless to say, entertains a doubt as to the integrity of the S.P.R. experimenters. But the same cannot be said of the subjects on whom they have experimented. It is for this reason that, *regarded as a means of convincing the world that the truth about telepathy has already been discovered*, the recorded experiments of the Society for Psychical Research are almost without value.

And yet that the world should be told, and *accept*, the truth on this vexed question is of incalculable importance. This is an age of transition ; an age of ever-increasing mental unrest. The relative numbers of those who receive their beliefs and opinions from their sires, without ever a thought of testing their validity, are yearly growing less. Never before in the history of man has the gnawing need of digging down to fundamentals obsessed so many minds as it does to-day. And everyone of the problems that thus harass and distract are found, in the end, to focus in the self-same question : Is matter the ultimate reality, or not ?

As said before, this overshadowing question would be answered—negatively, at any rate—if the truth about telepathy were once authoritatively settled. But how can this be done ? Who are there to act as humanity's trustees whom humanity will trust ?

Let us consider an excellent though quite unfeasible plan to start with. If the leaders of the two opposing parties interested in this question, say, Sir W. Crookes, Sir W. Barrett and Sir Oliver Lodge on the one side, and Sir Ray Lankester and Sir Bryan Donkin on the other, would give up all their other occupations and devote the next few years to an experimental inquiry on the subject of telepathy ; and if they arrived at a *unanimous* conclusion, no questioning of their verdict would ever find expression. But this, as it happens, being utterly impossible, it behoves us to look for an alternative solution.

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The S.P.R. was started just three years after Wundt had founded at Leipzig the first well-equipped psychological laboratory in the world. That was in 1879. Since then, many others have come into existence, in both hemispheres. In each is to be found a skilled staff of experimental psychologists, trained to bring into evidence any psychic trait a subject may possess, in spite of any nervous agitation that may mask it for a time. Now, if a faculty exists enabling one person to be the recipient of another person's thought without the intervention of sensation, who can say that, like any ordinary trait, it might not be liable to be similarly eclipsed? And if it were so liable, who could be better trusted, both to detect its presence and to bring it into play, than the trained and (presumably) impartial experimenters just referred to?

And, it may now be added, who so fitted as they to deal with the question before us? The ideal, no doubt, would be that all the psychological laboratories there are should commence experimenting simultaneously. For each being a check on every other, the likelihood of undiscovered fraud would be rendered practically *nil*. Should other than negative results occur in one, they and the methods that had been employed, could be communicated at once to all the rest, and a recipient that proved specially successful passed from one laboratory to another. This intimate co-operation could lead to but one result. The truth would inevitably emerge, and, which is far more important still, be universally accepted.

Then, in the name of all that is sensible, why not get the psychological laboratories to take on the job at once? But how? Nothing could be more simple. All that has to be done is to get the five gentlemen before mentioned to form themselves into a committee, and then induce that committee to set the ball a-rolling—and the thing is as good as done. A provisional memorandum as to the experimental methods to be employed, drawn up by them, would receive a warm welcome from the staffs of every psychological laboratory in existence. That with such names *in conjunction*, the ready assistance of the Press, both at home and abroad, could be confidently relied on, scarcely needs mentioning. It may, also, be taken for granted that such well-known students of this and kindred subjects as Dr. McDougall, Mr. Arthur Balfour, and Professor Henri Bergson would lend a helping hand in whatever way they

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could. The chief help they could afford would be to rouse the general public to the importance of the project. For, unhappily, psychological laboratories are not over-burdened with endowments. Our committee, consequently, could not ignore entirely the question of finance. However, that there should be any insuperable difficulty on that score cannot be imagined for a moment. A subscription list that was headed—and there is every reason why it should be—by the Royal Society and the British Association could not fail to prove sufficing. For Science is cosmopolitan, and France, Germany, Italy and the United States are equally interested with ourselves.

The future historian, writing a century or two hence on the development of mundane thought, will dwell on the settlement of this question (it will be settled long before the said historian is born) as a notable and outstanding landmark. No doubt, too, he will lay especial stress on the fact of it affording the only instance in the history of man of a high philosophic doctrine (Materialism) being recognised as disprovable by the experimental method.

That the human race, metaphorically speaking, is still in its childhood, cannot seriously be gainsaid. For, otherwise, the appalling diversity of opinion now prevailing on this planet would have passed away. Even how ridiculous it is that the mere accident of terrestrial latitude and longitude should determine a poor baby's life-long beliefs is not yet generally realised! It requires, however, no special understanding of the trend of evolution to know that this chaotic state cannot be a lasting one, and that, in time, the thinking of humanity will so effectually have *converged* that the basic teaching of tradition will be everywhere the same. Think of the lot of the more plastic minds when born in such a concordant world as that, and then compare with it their unhappy fate to-day. The lessons taught them in the nurseries *then* will stand them in good stead as long as life lasts, and not, as now, be bitterly unlearned in later adolescence.

To hasten the advent of this happier time, there is one step—a step that cannot but be fraught with immeasurable consequences—that *can* be taken. It is within our power to determine experimentally whether the human mind is an immaterial entity or not. If this were decided in the affirmative, materialists could no longer regard the unknowable something that holds space as the ultimate

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reality behind consciousness. In other words, Materialism would have received its death-blow. The fittest words to voice the feelings of Materialists at such a juncture are : Let truth prevail though the heavens fall.*

* This proposed inquiry, as far as materialists are concerned, would seem to be a case of "heads I lose, tails I don't win." If the human mind is proved to be an immaterial entity, their position becomes untenable at once. While if the verdict be in their favour, their opponents are merely dislodged from a single entrenchment, with plenty of others already dug to retire on. But this, of course, is quite irrelevant. For the only thing that each side wants—is truth.

THE SIGNIFICANCE OF THE DISCOVERY AT PILTDOWN

By Arthur Keith, M.D., LL.D., F.R.C.S.

ALTHOUGH the dust raised by the "hurly-burly" which followed the famous discovery at Piltdown is only in process of settling, the air is now clear enough to foretell with some degree of certainty how the issues of the day are to go. Mr. Charles Dawson and Dr. Smith Woodward brought *Eoanthropus Dawsoni* before the scientific public at a crowded meeting of the Geological Society on December 18th, 1912. Three months before then I had been accorded, in these pages,* the privilege of giving a brief account of "Recent Discoveries of Ancient Man," and of dealing with the bearing of those discoveries on our present knowledge of man's origin and evolution. A review of the conclusions reached in that paper will serve to show how far the discovery at Piltdown has obliged us to modify our conception of man's origin and antiquity. One of the conclusions reached in that paper related to the position of Neanderthal man in our ancestral tree, namely, that he was not, as is so often believed, the Pleistocene ancestor of modern man, but represented a totally distinct branch or species of humanity, one which became extinct in Europe some time before the end of the Pleistocene period—the geological epoch which precedes the present. If we accept the estimate of Professor Sollas that the Pleistocene period must have lasted at least 400,000 years, then we may, by way of a provisional estimate, regard Neanderthal man as having become extinct somewhere about 50,000 years ago—probably more. The conception of Neanderthal man as a distinct human species

* BEDROCK, October, 1912, Vol. I., p. 295.

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of man, marked by certain anthropoid features, was clearly enunciated by Professor William King,* of Queen's College, Galway, at the beginning of 1864; he named the species *Homo Neanderthalensis*. In the same year Huxley† carried the day against King, and hence, until the beginning of the present century, most of us regarded Neanderthal man as a primitive ancestral form of modern man manifesting particular affinities with Australian aborigines. Then with the present century appeared the investigations of Professor Schwalbe, Dr. Adloff, M. Rutot, Professor Sergi and Dr. G. Kramberger, with the result that King's conception began to replace that of Huxley. With the appearance, in 1913, of Professor Boule's elaborate monograph on the remains of Neanderthal man found at La Chapelle, within the watershed of the Dordogne, France, and the conversion of its author to the more reasonable view, we may regard the position of Neanderthal man as finally fixed. The discovery at Piltdown does not alter such a conclusion; it simply confirms us in believing that in the Pleistocene period there existed at least two very distinct species of mankind.

The discovery at Piltdown gave the death-blow to the "linear" theory of man's evolution—the conception that man had reached his modern estate in mind and body by a consecutive series of steps and stages. Until a few years ago, we saw, when we looked into the past, a single file of imaginary ancestors receding backwards in the geologist's scale of time, each more distant member of the file carrying us nearer to an anthropoid stage. The discovery at Piltdown was the final touch needed to scatter the "single file" conception. In my former paper in this magazine I sought to give clear expression to the idea that, when we seek to reconstitute in our mind's eye the world of ancestral man, the picture which we form must be based on, not the modern world of man, where a diverse but single species exists, but on the more primitive conditions seen in the world of anthropoid apes—man's nearest living allies. There are now living, in their own jungle districts of Africa and the Far East, three distinct genera of great anthropoid apes—the gorilla, chimpanzee and orang; we know of two extinct kinds—from France

* *Quarterly Journal of Science*, 1864, Vol. 1, p. 88.

† *Natural History Review*, 1864, Vol. 4, p. 429.

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and India—which cannot be regarded as ancestral forms of living anthropoids, but collateral forms. At one period there were at least five kinds of great anthropoids, probably many more. If we transfer to primitive man the picture suggested by his anthropoid allies, we have grounds for believing that at one and the same time there was not one species of mankind as at present, but many species and genera. Piltdown man is as different from Neanderthal man as the chimpanzee is from the gorilla; there were at least two species or genera of man living towards the end of the Pliocene and beginning of the Pleistocene periods in Europe.

Dr. Eugene Dubois' discovery of that peculiar humanoid form—*Pithecanthropus*—in Java some twenty-two years ago, opened the way for a belief in the multiplicity of genera in the world of ancient man. The evidence is now complete that this early human form survived in Java until late Pliocene or early Pleistocene times—the date assigned to Piltdown man. If *Pithecanthropus* be regarded as a direct ancestor of modern man, then we had to suppose that the human brain had almost doubled its size and complexity during the early part of the Pleistocene period—a rate of growth beyond the possibility of belief. The discovery at Piltdown brings us the positive proof that, as regards volume, one kind of man at least had attained a brain of almost modern size and development by the beginning of the Pleistocene. It is therefore plain that *Pithecanthropus* is an example—there are many parallel instances in the animal world to-day—of the survival or persistence of a very primitive form of mankind—one which, although surviving to the close of the Pliocene period, represents a form evolved during the geological period which preceded the Pliocene, the Miocene or even an earlier epoch. The outlook of the anthropologist has thus undergone a radical change during the last few years. In place of seeking, as formerly, to arrange all known extinct forms of man in a linear series, he is now endeavouring to discover the various branches in the great tree of man's ancestry—a tree with its roots buried deeply in the geological past, and its branches rising towards the geological present—and to assign each discovered extinct form to its appropriate place in this great ancestral tree.

Neanderthal man, as we have already seen, represents the terminal twigs of a dead branch of man's ancestral tree. What is the position

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of Piltdown man? Is he also a terminal twig of a dead branch or does his position lie directly on the stem of that branch of which all living races of mankind represent the terminal twigs? Does Piltdown man really represent our ancestor at about the beginning of the Pleistocene period—our ancestor some half-million of years ago? In their communication* to the Geological Society Mr. Charles Dawson and Dr. Smith Woodward leave this question unanswered. In his lectures on "Recent Discoveries of Early Man" at the Royal Institution, Dr. Smith Woodward was, however, more definite. In his opinion the Galley Hill and Ipswich skeletons are "now to be regarded merely as late burials." It is clear from this statement that Dr. Smith Woodward is of opinion that Piltdown man is our Pleistocene ancestor and that all those remains of men of the living type which have been found in undisturbed strata, laid down during the middle third of the Pleistocene period, are really the bones of comparatively modern individuals, buried in rather ancient strata. A survey of the evidence before the Piltdown discovery had led me to formulate, in these pages, an exactly opposite conclusion, namely, that the discoveries at Galley Hill, near Gravesend, at Bury St. Edmunds, at Ipswich and others, made in France and in Italy, were guarantees that by the middle third of the Pleistocene period, men of the modern type were already evolved. Since then I have again gone over the geological evidence bearing on these human remains, and I see no loophole of escape from the conclusion that these remains are as old as the strata under which they were found.

Any one enquiring into the ancestry of modern man will find the discovery of the Ipswich man in 1911 and the Piltdown man in 1912 very instructive. Both were found beneath shallow deposits—within easy reach of the gravedigger's spade. The skeleton of the Ipswich man lay on a Pleistocene deposit—the mid-Glacial sands, and under an unbroken layer of weathered chalky boulder clay, also a Pleistocene deposit—only 4 feet in thickness. The remains of the Piltdown skull came from an iron-cemented stratum of gravel, only about 6 inches thick, lying beneath a layer of gravel rather less than 4 feet in depth. At Ipswich, as at Piltdown, the remains lay scarcely 5 feet beneath the surface.

* *Quarterly Journal of the Geological Society*, 1913, Vol. 69 (March), p. 117.

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Why is it, then, that the Ipswich skeleton has been rejected by the vast majority of geologists while the Piltdown remains have been universally accepted as ancient and authentic? The answer is not far to seek. Except as regards his shin bone or tibia, the Ipswich man was like modern man; in every feature of his skull, jaw, and teeth Piltdown man differed from modern man. The peculiar characters of the Piltdown man gives him a free pass to our confidence, but why do we reject the man from Ipswich? Can he not be ancient, even if he is not marked by peculiar characters? My friend, Dr. Ales Hrdlicka, of the Bureau of American Ethnology, has expressed very well the attitude of most geologists and anthropologists to such discoveries as that at Ipswich. Six years ago he published a most valuable account* of the various discoveries of ancient man which had been made in North America. He had made a personal examination of each one of them, and in not a single case was he convinced of their authenticity—chiefly on the score that the human remains thus discovered differed in no material circumstance from those of modern man. I shall cite an instance. In 1902 an adult skeleton was discovered at a depth of 23 feet in a Glacial deposit at Lansing, Kansas. Dr. Frederick Wright, who has given a lifetime to the study of Glacial deposits in North America, regards the Lansing deposit as formed before the last cycle of glaciation and gives its probable antiquity as 12,000 years. If the European cycles of glaciation were contemporaneous with those of North America, then the antiquity, if I may infer from the estimates given by our own geologists, is three or four times greater than that given by Dr. Wright. No one has ever called in question, no one can call in question, that the Lansing skeleton is as old as the deposit under which it lay. Dr. Hrdlicka, however, rejects it on the ground that "this man was physically identical with the Indian of the present time," and to accept the skeleton as authentic would involve "the far more difficult conclusion that his physical characteristics during all the thousands of years assumed to have passed have undergone absolutely no important modification." We have here the expression of a belief widely accepted

* "Skeletal Remains suggesting or attributed to Early Man in North America," Smithsonian Institution, Bulletin 33, 1907.

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by modern biologists who regard the law of change as so dominant that it is almost impossible for any animal species to come through a whole geological period and remain unchanged. Dr. Smith Woodward assumes the same attitude towards the Galley Hill remains as Dr. Hrdlicka takes towards the Lansing skeleton. The hard case of modern man is thus apparent : he is, in a geological sense, sentenced before he is tried.

Before we can answer the question, Is the Piltdown man our direct ancestor ? we must make two preliminary enquiries : When did he live ? What sort of person was he or she ? If we find in him just those features which foreshadow our own, and if it can be proved that he lived at the same time as the Galley Hill man,

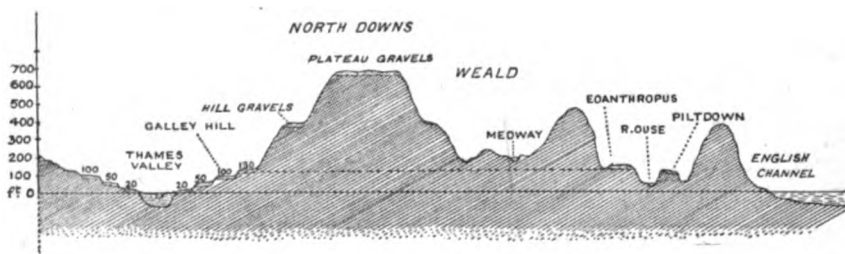


FIG. 1.—Diagrammatic section across the Thames Valley and Weald of Sussex (after Hinton and Kennard ; also Mr. Charles Dawson).

then most of us would be inclined to suspect the authenticity of the Galley Hill find—to suspect that in some astute manner Mr. Robert Elliott, Mr. Matthew Heys, Mr. E. T. Newton and Dr. Frank Corner had been tricked. In order to make clear the accepted beliefs regarding the ages of the Galley Hill and Piltdown men I have given a diagrammatic section beginning at the Thames Valley and running southwards through the Weald of Sussex, cutting both the North and South Downs. The diagram is compiled from papers and sections published by Mr. A. C. Hinton and A. S. Kennard* and by Mr. Charles Dawson,† the discoverer of Eoanthropus. The Galley Hill skeleton was found in the 100-foot terrace on the south side of the Valley of the Thames at a depth of 8 feet ; the bed of

* "The Relative Ages of the Stone Implements of the Lower Thames Valley," *Proc. Geol. Assoc.*, 1905, Vol. 19, p. 76.

† *Quarterly Journ. Geol. Soc.*, 1913, Vol. 69, p. 117.

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gravel in which the Piltdown remains were found lies within the Weald, and occurs at a higher level than the 100-foot terrace of the Thames. The gravel on the Piltdown plateau lies about 120 feet above the datum line of the Ordnance Survey, and, therefore, as regards level, represents an older and higher terrace—the so-called 130-foot terrace—than that in which the Galley Hill skeleton was found. Mr. A. S. Kennard, however—and his opinion must carry weight—regards the Piltdown gravel as of the same age as the 100-foot terrace. Mr. Reginald Smith and Mr. Henry Dewey have recently given us accurate information regarding the cultural ages which are represented in the 100-foot terrace.* The deeper or older strata were laid down where the natives of the Thames Valley—the bed of the river was then flush with the 100-foot terrace—were working their flints in the pre-Chellean or Strepyan style; in the middle strata come the products of the long Chellean age, and in upper or more recent layers both periods of the Acheulean culture appear. Apparently the 100-foot terrace began to be formed when mankind was in the Strepyan stage of culture, and its formation ended with the Acheulean stage; its strata are mid-Pleistocene as regards geological age. The gravel deposit at Piltdown is a shallow one, but there are two strata in it. In the distinctly-marked bottom stratum were found the remains of *Eoanthropus*; in the same stratum were found the remains of animals—*Stegodon*, *Mastodon*, *Hippopotamus*—which lived during the Pliocene epoch; remains of a beaver were also found, and it, for aught we know to the contrary, may also be Pliocene in date. Indeed, Mr. Lewis Abbott, who knows the recent geology of the Weald as well as any one, has no hesitation in assigning the bottom *Eoanthropic* stratum to a Pliocene date. Ever since Prestwich's time we have regarded eoliths as the handiwork of Pliocene man; eoliths occur in the bottom stratum; there is, therefore, presumptive evidence that the bottom stratum is Pliocene in date and that *Eoanthropus* represents a form of Pliocene man. In the opinion of Mr. Dawson and Dr. Smith Woodward, both the bottom and upper strata at Piltdown are early Pleistocene deposits. In the upper or disturbed stratum were found certain worked flints which they regarded as

* "Stratification at Swanscombe," *Archæologia*, 1913, Vol. 64, p. 177.

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of the Chellean type of workmanship. If the upper bed is of the same age as the deep stratum, and if these flints are really of the Chellean type of workmanship, then Eoanthropus and the Galley Hill man were contemporaneous—both belonging to the middle Pleistocene period.

It is evident, however, that neither Mr. Dawson nor Dr. Smith Woodward place any reliance on these worked flints, for they assign Eoanthropus to an early part of the Pleistocene period—to a time when a much more primitive culture than the Chellean was in existence. Eoanthropus, they think may have been a contemporary of the Heidelberg man. To show how far the Heidelberg man antedates the Chellean period I have reproduced a figure from my former paper in these pages. In M. Rutot's opinion, two long cultural phases intervene between the time of the Heidelberg and Galley Hill men. All the evidence then leads us to suppose that Eoanthropus precedes the Galley Hill man by a long space of time. How long we have no means of forming an exact judgment, but on the rough methods at present in use we are certain that it amounts to many thousands of years. If Eoanthropus was a contemporary of the Mastodon—which he may have been—then he takes his place in the Pliocene, and his date is still further removed from that of the Galley Hill man.

Mr. Charles Dawson not only discovered Eoanthropus; the existence of an early Pleistocene or late Pliocene deposit in the Weald of Sussex was also his revelation. Deposits of an early Pliocene date were known on the uplands of the North Downs, but no geologist had ever guessed that down in the bosom of the Weald there lay deposits which are either late Pliocene or early Pleistocene in date. It was Mr. Dawson that revealed such a formation. No one has made a more systematic examination of the Pleistocene and Pliocene deposits of Western Europe than M. Rutot of the Royal Natural History Museum of Brussels. On hearing of the discovery of Eoanthropus he asked me at what level the Piltdown gravel occurs, and how far it lay above the level of the Sussex Ouse. I replied, 120 feet above sea level, and 80 feet above the Ouse. His answer was: "That deposit is Pliocene in date; it is nearly of the same age as the deposits at St. Prest, on the Eure, near Chartres, France." I immediately became interested in St. Prest. On consulting a

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map, which Professor Boyd Dawkins had prepared to show the state of Western Europe in late Pliocene times, I found that the Sussex Ouse and the French Eure were tributaries of the same great

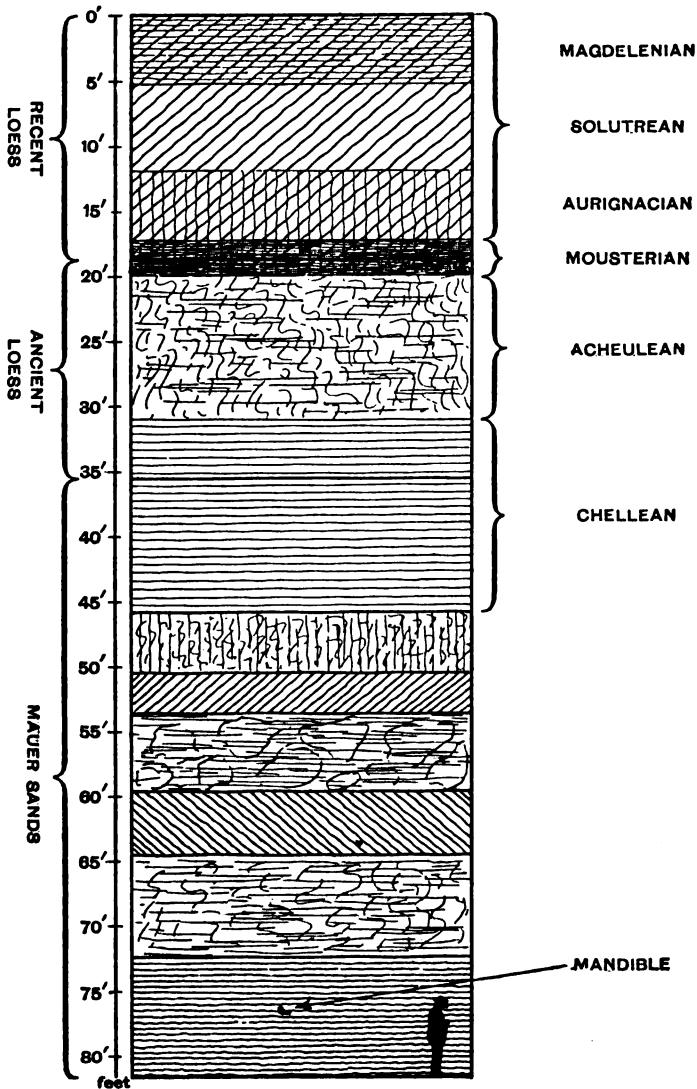


FIG. 2.—Diagrammatic section of the strata of the sand-pit at Mauer, where the Heidelberg Mandible was found. The cultural periods corresponding to the upper strata are indicated, but Rutot's Strepyan, Meevnian and Mafflian cultures of the lower strata have been omitted.

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river flowing westwards on the site of the English Channel. The deposits at Piltdown and at St. Prest thus lie within a common watershed; in each case, both at Piltdown and St. Prest, the neighbouring river has deepened its channel 80 feet below these old gravel deposits. About the geological position of the deposits at St. Prest there has never been any doubt; they are late or upper Pliocene. In April, 1863, M. M. J. Desnoyers,* described by Hamy as "le savant bibliothécaire du Muséum," discovered that many of the fossil-bones found even in the deeper strata at St. Prest showed exactly the same markings as bones from Pleistocene caves—markings which were regarded, as far as the cave bones were concerned, as undoubted evidence of man's handiwork. Only the fact that these evidences of man's work occurred in a deposit of Pliocene date prevented Desnoyers' discovery from being universally accepted. Since Desnoyers first announced his discovery worked flints have been found in the St. Prest deposits. In M. Rutot's opinion the "Prestian" flints are of a later date—a higher type—than the Kentish flints or eoliths found in the bottom stratum at Piltdown with *Eoanthropus*. The fauna at Piltdown is of an older Pliocene complexion than that of St. Prest. Westwards from Piltdown, near the Dorset village of Dewlish, lying also within the watershed of the ancient Channel river, is another piece of evidence which may throw some light on the condition of man towards the end of the Pliocene period. This evidence takes the form of a deep, narrow trench cut in the chalk, 100 feet long, 12 feet in width, which contains the remains of a Pliocene form of elephant (*Elephas meridionalis*), and also flints, which Mr. C. J. Grist† regards as of the same workmanship as the Kentish eoliths. The Rev. O. Fisher,‡ who investigated and described this trench in 1888, cannot conceive how it could have been fashioned unless by the hand of man. It is similar to trenches dug by certain native tribes for the capture of elephants. Taking the evidence at Dewlish and St. Prest into account, we seem to have reliable indications of a fairly high type of man at the end of the Pliocene period.

* *Compt Rendu. Acad. des Sc.*, 1863, Vol. 56, pp. 1073, 1199.

† *Journ. Roy. Anthropol. Instit.*, 1910, Vol. 40, p. 192.

‡ *Quart. Journ. of Geol.*, 1888, Vol. 44, p. 819; 1905, Vol. 61, p. 35; *Nature*, 1913, Vol. 92, pp. 6 et seq.

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Having thus tried to ascertain all that can be known at present of the age or period of the Piltdown remains—the evidence pointing to a Pliocene rather than a Pleistocene date—we shall now seek to answer as briefly as possible : What kind of person was he or she ? We may take the question of sex first. It is often hard enough on the evidence of the skull by itself, to recognise the sex of even a modern person ; it is much more difficult, well nigh impossible, to recognise the sex marks in a new type of human being represented by only a fragmentary skull of one individual. Dr. Smith Woodward is inclined to regard the Piltdown skull as that of a woman ; the markings, in my opinion, point to the male sex. The head is massive,

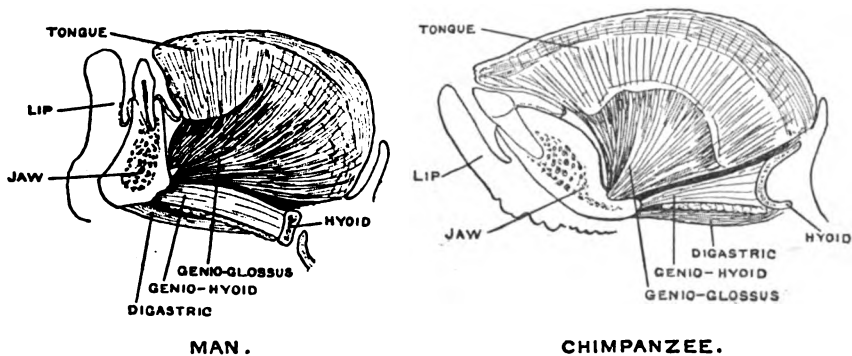


FIG. 3.—A. Section of the tongue and chin region of the lower jaw of modern man. B. Corresponding parts of a young chimpanzee.

the mastoid processes are not only well marked but large ; the neck was thick, for the mastoid processes which mark the width of the neck, behind and below the ears, are wide apart. Perhaps the safe way, until we get a fuller knowledge of the Piltdown race, is to speak of this individual as “ it ” rather than “ he ” or “ she.”

Early in the month of November, 1912, I had the privilege of seeing the Piltdown fragments for the first time. I was duly impressed with the remarkable thickness of the bones which form the brain chamber of the skull, and their degree of fossilisation, but the feature which seized my attention was the chin ; never before had a chin with such characters been seen in a human being—only in apes. The two accompanying figures (Fig. 3) will make this remarkable feature clear. On the hinder aspect of the lower jaw in the region

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of the chin is a deep pit, from which the chief muscle of the tongue arises—the genio-glossus. This pit is bounded below by a shelf or plate of bone—we may call it the “simian chin-plate.” That plate is present in the Piltdown lower jaw; the upper part of the jaw in the region of the chin is broken away, but we may safely assume that in this region the Piltdown jaw was very similar to that of an ape. In modern man the chief muscle of the tongue no longer arises from a pit but from a bony prominence (Fig. 3). We know what must have happened in the lower jaw of man in passing from a simian to a human stage of evolution, for the Heidelberg mandible (early Pleistocene) shows us an intermediate stage. In that mandible the pit is nearly filled up with a new growth of bone, forming the prominence from which the chief muscle of the tongue arises. In the filling up of the pit the simian plate seems to disappear; in reality it is included in the prominence from which the tongue muscles arise. In modern man a further change has occurred; the chin is no longer receding but prominent (Fig. 3, A). The prominence of the modern chin is the result of two changes: one of these is the great diminution in the size of the teeth, with the result that the upper or tooth-bearing part of the lower jaw recedes backwards, leaving the chin prominent. The prominence of the chin, however, is also due to another factor—one related to speech. To facilitate speech the floor of the mouth, which is bounded by the lower margin of the jaw, must be widened and opened out, in order that the tongue may be freely elevated and depressed during speech, for the tongue is the chief organ of articulation. I regard all the essential features of the human lower jaw and chin as adaptations to the faculty of speech; I know of no other explanation for man’s peculiar chin characters. If my reasoning is right, then the Piltdown man was not adapted for articulate speech; it may be questioned if even the Heidelberg person had this faculty developed to more than a rudimentary degree.

The presence of a simian chin-plate in the Piltdown mandible seems to me to throw some light on the probable period of the race to which it belongs. I could not conceive that the extraordinarily elaborate and complex mechanism which underlies the faculty of speech could have been evolved independently in two or more ancestral races of mankind. One can see that the process of

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adaptation to speech was already under way in the Heidelberg man early in the Pleistocene period ; in the Piltdown mandible that stage has not been reached ; the condition is still simian, and I infer that Piltdown should be much earlier than Heidelberg—at least Pliocene in date. It is also possible, as Dr. Smith Woodward suggested, and I was keenly alive to the possibility, that the Piltdown race was a survival from a more ancient time, which had persisted in England when mankind in another part of the world had made some progress in the faculty of speech. If Piltdown man is Pleistocene in date that is the only feasible explanation.

The presence of that simian chin-plate in the Piltdown jaw, together with certain other minor features, led Dr. Smith Woodward to infer that such characters must also be accompanied by a simian dentition—at least, as far as regards the front teeth. When, therefore, he reconstructed the mandible and supplied the missing parts, he gave the Piltdown race massive projecting simian canine teeth—teeth like those of a chimpanzee. It was a perfectly legitimate inference, but there were certain features which seemed to me to render the presence of pointed, projecting canine teeth impossible. Projecting canine teeth prevent a free side-to-side movement of the lower jaw in chewing—such as is seen in animals which chew the cud, and also in man during normal mastication. In all animals—man or horse—which have this side-to-side chewing movement, the canines are not prominent, and the jaw is peculiarly jointed to the skull. The articular condyles of the jaw rest in a cavity, the glenoid cavity ; as the jaw is carried to the left side, the right articular condyle moves from the cavity on to the articular eminence which forms the front part of the glenoid cavity. That articular eminence is particularly well developed in the Piltdown skull—better even than in modern man. It therefore seemed to me—still seems—that the canine teeth could not have been projecting. In no known mammal are projecting canine teeth accompanied by, or correlated with, an articular eminence. But it is true, as in Neanderthal man, the articular eminence may be absent and yet projecting canine not be present. Another fact which influenced me was that the two molar teeth, still preserved in the jaw, seemed worn by such a side-to-side movement ; a third factor was the relatively small size of the temporal

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muscle which rises from the side of the skull and acts on the jaw. The extent and size of this muscle can be inferred with some degree of certainty from the markings on the side of the Piltdown skull ; the muscle was distinctly smaller than in modern Australian aborigines. A large, prominent canine seems to demand the need of a large temporal muscle ; signs of such a muscle are absent in the Piltdown specimen. A fourth character which weighed with me was that in all anthropoids—they have all large projecting canines—the skull is especially firmly hafted to the neck, in order, at least so I infer, that these animals may exercise their ferocious bodily strength through their jaws and great canine teeth. For this reason the mastoid processes of anthropoids do not form downward-projecting knobs, behind the ears, as in modern man, but are thrown outward, as wide bony flanges, to give additional room for the insertion of the strong muscles of the neck. In the Piltdown race the skull was hafted to the neck as in modern man. For all those reasons I concluded that the law of correlation, as propounded by Dr. Smith Woodward, did not hold true, and that the canine of the Piltdown race would be found to be massive but shaped as in modern man.

By the end of the summer of 1913, however, a canine tooth was found in the bottom stratum at Piltdown not far from the spot at which the fragments of the skull were recovered. It was of the type postulated by Dr. Smith Woodward—a pointed simian canine. The tooth was of the right side, it was the corresponding half of the mandible which had been found. In his reconstruction of the jaw, Dr. Smith Woodward had given the canine a “ back to front ” diameter of 14·5 mm. ; I had, in mine, allowed it 10 mm.—2 or 3 mm. more than in the large modern canine teeth ; in the tooth found this diameter was 11 mm. The crown of the canine was deeply worn, but not by attrition against the upper canine, which must have been even more projecting than the lower, but by the upper lateral incisor tooth—a type of wear occasionally seen in the canine teeth of anthropoids. The discovery of this simian-like tooth and part of a simian-like mandible near to each other, and within the same stratum, carries home the conviction to us that they must be parts of one animal, for one cannot suppose that some strange coincidence had brought side by side the canine

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tooth of a man-like ape and the lower jaw of a man with certain ape-like features. But all the same, this lately discovered tooth presents two features which raise a suspicion as to whether or not it does belong to this particular mandible. Its colour is of a very much darker shade than that of the two molar teeth still in place in the jaw ; it also shows signs of long and of hard wear. That is a remarkable fact, because in the Piltdown jaw, although only the socket of the third molar or wisdom tooth is present to bear witness of the fact, we know that the third molar was not completely erupted—had not come into use. The X-ray picture of the jaw demonstrates that fact. Now in all anthropoid jaws the canine and third molar teeth cut or erupt about the same time. We must presume, from the characters of the canine teeth and of the mandible itself, that the canine or third molar teeth should erupt about the same time in the Piltdown race. To me it seems an impossibility that the canine could be worn to such a degree and the third molar tooth not erupted in one and the same mandible. The condition of the third molar gives us the most definite evidence that the Piltdown jaw belonged to an immature individual. The canine tooth, on the other hand, is that of a quite mature, even aged, individual. The case of the third molar or wisdom tooth in man may occur to the reader ; its eruption is often delayed in modern man ; sometimes it never cuts at all. The delay of the wisdom teeth is a recent feature of man's dentition, for I have failed to find a single instance in either ancient or primitive man in which there was any evidence of the delay or non-eruption of the wisdom tooth. In the Piltdown mandible we are dealing with a very primitive form of human dentition.

Even if the tooth does not belong to this particular mandible we must suppose it belongs to one of the same kind. That a human ancestor should be discovered with projecting ape-like teeth has been anticipated by all who have closely studied the anatomy and development of man's teeth ; one cannot account for the peculiar history of our canines unless one supposes they have come through a simian stage. But I expected to find, when that simian stage in our ancestors was discovered, that the other anthropoid accompaniments which are seen in the mandibular joint and in the muscles of mastication, would also persist with the projecting canines. In the

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Piltdown skull all the features which suggest a human form of mastication are present—those features on which I have already laid stress. There is one way out of this difficulty—that suggested by Sir E. Ray Lankester and urged by Professor Waterson—namely, that the mandible and the skull are parts of different kinds of beings; the mandible that of some unknown anthropoid and the skull that of a primitive form of man. When we seek to get out of our difficulty in this way we raise others. The molar teeth in the Piltdown mandible are essentially human in appearance; the texture of the mandible is similar to that of the skull. The markings for the temporal muscle, which acts on the jaw, are different to any ever seen in a human skull and indicate that the mandible should be of a peculiar character—such as has been found. For my part I am trying to see if it is possible that an articular eminence could exist with projecting canine teeth. Amongst known mammals there is no instance of projecting canines and articular eminences existing together in the same individual. Still, there may have been some peculiar arrangement of the canine teeth which permitted side-to-side movements in the Piltdown race. In the meantime I am proceeding on the assumption that the future will bring forth the evidence which is needed to show that the Piltdown combination of characters is possible. The existence of such a combination, however, must not be regarded as proved.

At the celebrated meeting in the Geological Society's rooms on December 18th, 1912, I had no fault to find with the manner in which the Piltdown skull had been restored. When replicas of the cranial fragments were distributed in May, 1913, I had no idea that any criticism could be made on the manner in which the work of restoration had been carried out. My attention was first drawn to Dr. Smith Woodward's reconstruction when I had worked out on a fairly large modern skull, the parts which had been found—in order that visitors to the Surgeons' Museum might realise the position and size of the actual parts on which the reconstruction was based. The skull I worked on had a capacity of 1,450 c.c.—a little below the average capacity for Englishmen, which is about 1,490 c.c. I found that the Piltdown fragments were parts of larger bones than those in the skull on which I worked. The capacity of the Piltdown skull being only 1,070 c.c.—a

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low capacity, the fragments should have been considerably smaller than the corresponding parts of the modern skull—even when allowance is made for the fact that the thickness of the Piltdown bones is nearly twice that which obtains in modern skulls. On searching for an explanation of this discrepancy, I found that there was a great degree of asymmetry in the restored skull—that the left and right halves did not match. In the brain cast the right and left halves were markedly unlike. Instead of measuring only 1,070 c.c. as had been announced, the brain cast displaced almost 1,200 c.c. of water. The left half in its posterior part was 90 c.c. greater than the right. Now in all primitive forms—in anthropoids and primitive races of men—the right and left halves of the skull and brain are approximately symmetrical in size and form ; symmetry is a primitive character therefore to be expected in an early type of man. In searching for the cause of the asymmetry I found that it was chiefly due to a wrong articulation of parts. It may be urged against me that in a new form of being, such as that we are now dealing with, the laws which hold good for modern human skulls may not be fully applicable. That cannot be put forward as a valid reason, for Dr. Smith Woodward announced that he could detect no marked difference, except as regards thickness, between the Piltdown and modern cranial bones. With that opinion I do not agree ; every one of the Piltdown cranial bones has distinctive characters—characters which mark them off from the same bones of modern man. But they are of a truly human shape, and, therefore, in putting the parts together, we must proceed in the task of restoration exactly as we would if the corresponding parts of a modern skull were before us. When the parts are articulated, so that each structural point occupies its normal anatomical position, the asymmetry of the restored skull disappears to a large extent, and the capacity of the skull becomes, not 1,200 c.c., but 1,500 c.c., that amount being rather above the modern average. The Piltdown skull is in reality massive. There are several methods in which one can proceed in the act of reconstruction ; I checked one method against another, and found that in each experiment—by employing a totally different procedure—the result came out the same in each case.

The results of a long and close investigation of the Piltdown skull

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has led me to the following conclusions:—(1) that the remains found at Piltdown represent a form of man living in the later part of the Pliocene period ; (2) that he had a massive skull, differing from modern skulls in many structural characters ; (3) that the brain was as large as in the average of modern races ; (4) that the jaws and teeth were more anthropoid than human in character ; (5) that, although the brain is large, I cannot believe that speech was possible with such a conformation of jaw and tongue ; (6) that, on the whole, the evidence is in favour of the mandible and skull being parts of one individual, but that the canine tooth belongs to another individual of the same race.

As regards the final question, Have we found at Piltdown an ancestor of the modern races of mankind ? my answer is in the negative ; Piltdown man is apparently not our ancestor. The facts on which the solution of the problem has to be based are few and fragmentary ; we have to frame our explanation to account for such facts as we have now at our disposal. We have, in the first place, the early Pleistocene Heidelberg jaw ; that tells us for certain that a man of the Neanderthal type was in existence at that time—Neanderthal man with great brow ridges similar to those of the chimpanzee and gorilla. So closely are these two anthropoids allied to man structurally that we must suppose the gorilla, the chimpanzee, Neanderthal man, Piltdown man, and modern man—to have a common origin—a common ancestor. It is unlikely that those peculiar eyebrow ridges arose independently in each genus. We may set it down for certain that simian eyebrow ridges were a character of the common ancestor from which all human races, human species and human genera have arisen. Neanderthal man has retained them ; Piltdown man and modern man have lost them. We must also presume that the simian chin-plate is a common inheritance—a character of man's common ancestor. In Piltdown man this plate has been retained as in the gorilla and chimpanzee ; in Neanderthal man, as in modern man, it has been lost. We must presume, then, to account for certain facts, that the common ancestor of human races possessed simian eyebrow ridges and a simian chin-plate. From that common ancestry springs one form, in which the eyebrow ridges were dominant and persisted, but the chin-plate was recessive and disappeared, the combination found in

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Neanderthal man. In another form, the eyebrow ridges were recessive, but the chin-plate was dominant, the combination found in Piltdown man. We may presume, independently of corroborative facts—the discoveries of remains of the modern type of man, at Castenedolo (Italian Pliocene), at Olmo (Italian early Pleistocene), at Galley Hill (mid-Pleistocene)—that there was a third form evolved at the same time as the other two, one in which both eyebrow ridges and chin-plate were recessive. This third form would give us the ancestry of modern races of men. Far from proving, as Dr. Smith Woodward evidently thought, that the modern type of man could not have existed at the time the 100-foot terrace of the Thames valley was deposited, the discovery at Piltdown has furnished us with the strongest evidence in favour of his early, probably Pliocene, evolution.

VITALISM :—AN OBITUARY NOTICE

By Hugh S. Elliot

MY controversy with Dr. McDougall on the subject of Vitalism is now complicated by a new point of view and a new warrior in the person of Dr. C. A. Mercier. Dr. Mercier takes up the attitude that the problem is insoluble: he holds, he says, a detached position, disagreeing both with Dr. McDougall and myself. In his article, however, he confines himself to a somewhat vigorous attack upon me, while leaving Dr. McDougall entirely alone. Perhaps, therefore, it is natural that I should fail to see where the detachment comes in. If he is equally opposed to Vitalism, why does he not endeavour to criticise Dr. McDougall's position also? It appears to the outsider that Dr. Mercier is a thorough-going Vitalist in spirit, while having a very natural reluctance to associate himself with a school, of whose weaknesses he must be conscious. However that may be, the solution presented by Dr. Mercier seems on the whole less tenable than any other solution yet put forward by either side. But before proceeding to a discussion of the matter of these articles, let me say a few words as to their manner.

Dr. McDougall in his solitary criticism of my last article in *BEDROCK* (July, 1913), affirms that I am too obtuse or disingenuous to take his point: and gaining courage as he goes along says that I stand convicted of extreme obtuseness or wilful misrepresentation. Dr. Mercier likewise submits that I flap my wings, and speak in the voice of Chanticleer: which (as I gather from the *Oxford Dictionary*) is a personal name for the domestic cock. Let the reader remember that what I have endeavoured to do is to defend a doctrine very generally recognised among men of science: a

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doctrine which among physiologists is not often even questioned. And up to date the proceeds of the controversy, so far as I am concerned, consist in the representation of myself as an obtuse and disingenuous variety of domestic fowl.

From such charges I do not care to defend myself : I leave my personal case with entire confidence in the hands of my readers. They will, I know, estimate Dr. Mercier's zoological parallels for precisely what they are worth. They will not expect from me that I should seek inspiration for a rejoinder by a visit to the Zoological Gardens. All they will expect from me is that I should reply to the actual arguments employed by Dr. Mercier against Mechanism. I fail completely to understand on what Dr. Mercier bases his statement that my own "expressions go beyond the limits of fair controversy" ; in passing, I merely note his opinion that comparison of your opponent to a farmyard fowl does not go beyond those limits.

Nor do I feel in any way discouraged by such an analogy. I am no sceptic, like Dr. Mercier. I am defending a scientific doctrine, which few physiologists question ; but which they do not advertise in the newspapers, because they do not care sufficiently whether the general public believe in it or not. I have myself brought forward a series of arguments in favour of that doctrine, which in common with many others I believe to be quite unanswerable. I have pressed Dr. McDougall more than once to answer them : I have numbered each argument (1), (2), (3), etc., for greater facility of reply, and to ensure that none should be left out. But in vain ! I cannot get an answer. I cannot induce him to join issue on any fundamental question. Am I not, then, justified in stating that the orthodox scientific doctrine represents a truth, whose main features are beyond the range even of criticism by their opponents ? And if it is true, am I not justified in defending it with such power of language as I may be able to derive from the consciousness of devotion to the mighty cause of scientific progress and truth ?

I note, however, that it is in my own interests that Dr. Mercier invites me to moderate my expressions, for he thinks they go beyond the limit of "effective controversy." I am indebted to him, therefore, for his advice that a more effective reply may be given by using milder methods. I cannot but admire the disinterestedness of an opponent who gives you hints as to how he may be most

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effectively answered—the recipe being, apparently, to hit him quite softly.

Dealing now with the substance, first, of Dr. McDougall's article, there is little more to be said. In my first article of the present controversy (October, 1912), I set forth a number of points which, if well-founded, constituted a proof of scientific mechanism: and I invited Dr. McDougall to criticise such parts of my foundation as he considered unsound. In his reply, he ignored the greater number of my points, and raised others. In my further rejoinder (July, 1913), I took some pains to comment on each of the new points he had raised, though I was not always conscious how they affected the discussion. At the same time I re-enumerated all those points from my first article, which Dr. McDougall had completely ignored. Placing them, as I did, under separate headings, and seeing that they constitute, as they stand, an overwhelming case against Vitalism, I thought it hardly possible that Dr. McDougall could continue to ignore them and at the same time continue to believe in Vitalism. But this he has done. He refers to one only of my points (and to that I shall come in a moment). He then goes on to affirm that I stand "convicted in this matter of either extreme obtuseness or wilful misrepresentation. I will add that this one instance of his [my] criticisms and of his dealings with my replies seems to me a very fair sample of the whole of them." In short, Dr. McDougall absolves himself from giving replies to my points, on the ground of my personal moral obliquity. Well, let all this stand for the sake of argument: let it be admitted that I am obtuse, that I misrepresent Dr. McDougall, that I am disingenuous, and that I am guilty of any other similar accusations which Dr. McDougall may hereafter desire to apply to me. Let us suppose all these accusations to be true: what then? Does it alter the matter, that I *did* name certain facts and arguments, which, if true, were conclusive against Vitalism, that I asked for an answer, and got none? Does it alter the case that, in writing a second time, I again recited these facts *seriatim* and pressed for an answer, and again got none? Whatever moral obliquity may attach to me, whatever obtuseness I may exhibit, however undeserving of reply I personally may be, these facts remain unanswered, and until they are answered Vitalism stands condemned.

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And now what am I to do ? Am I to recite for the third time those same facts, which already for the first time and the second time I have exposed with all the force in my power,—with a force that has excited the sympathetic wrath of Dr. Mercier ? What is the good ? If Dr. McDougall had any replies he would have given them before : and if he still wishes to give them, he has but to look back to my last article. But I know he will not reply, for I know he cannot. As I said in my last article, “ *Vitalists will not meet the crushing arguments opposed to them, because they cannot meet them.*” In reply to the article containing this citation, Dr. McDougall names one point as illustrating my obtuseness, and says that it is a sample of the rest, which thereupon he never mentions.

And now as to the one point. It concerns automatic writing. Dr. McDougall says that I persist in my misinterpretation of his opinion, in accrediting him with the belief that “the mere occurrence of automatic writing” is “evidence of a future life.” I never said anything about a future life, and intended to raise quite a different point. I do not see still how I misrepresented Dr. McDougall, but he must know what he meant better than I do ; and if he says that I misrepresented him, I must accept his statement, and tender my apologies.

I have but little to add on the subject of Dr. McDougall’s article. Perhaps it is true, as he says, that our minds are so hopelessly separated that they cannot come into effective contact. Yet the canons of logic are not subjective : they should be the same for all minds alike. A fact is a fact, however far our minds may be removed. Dr. McDougall now quotes Dr. Merz and Mr. L. T. Hobhouse in favour of his views, saying that I will regard any reasoning of his own on this point as vitiated by sentimental prejudice. I think he must have misunderstood me. If I selected his writings for criticism rather than those of Dr. Merz and Mr. Hobhouse, it was because I thought him a more formidable opponent than they, and better acquainted with the details of physiology. So far as I am concerned, Dr. McDougall’s case does not gain in weight by any citation from these or other writers : for I know well and have said many times that the vitalistic point of view is much in vogue among philosophers. Should an appeal to authority be considered desirable, however, I think the names of

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Dr. Merz and Mr. Hobhouse are more than overbalanced by those of Sir Ray Lankester and Sir Edward Schäfer. I here conclude what I have to say about Dr. McDougall's articles: nor, unless circumstances require it, do I propose to renew the subject. On my side, at all events, I shake hands with Dr. McDougall, and thank him for what has been to me a very interesting discussion.

I now turn to the article of Dr. Mercier. I confess that in some ways I find his article more difficult to answer than that of Dr. McDougall:—not, let me hasten to add, because it is more substantial: but because the negative position assumed by Dr. Mercier is necessarily more intangible than a positive position such as that assumed by Dr. McDougall. Moreover, Dr. Mercier is a keen and experienced controversialist, who thoroughly understands all the strategy and tactics of the art: and before taking his aim at me, he intrenched himself in a position where he evidently thinks he cannot be reached.

Dr. Mercier's contention is that Vitalism is only appropriately dealt with from the agnostic standpoint. I hold the positive view that the material cerebral operations, resulting in muscular and glandular activity, are wholly and completely independent of any influence of spiritual character. Dr. McDougall apparently still holds the equally positive view that there is some spiritual intervention in the material cerebral operations. Dr. Mercier now rushes in with the negative or agnostic view. He says it is impossible to say which side is right: he lays claim to the position of absolute ignorance on the matter.

Now this claim, so far as Dr. Mercier himself is concerned, is one that I have no intention of contesting. If he had confined his article to an exposition of his own personal attitude, if he had done no more than affirm his individual ignorance and the improbability that that ignorance will ever be removed, I should then have regarded his essay as a triumphant success, and the character of his arguments as a complete vindication of his views. But he does not confine himself to these limits: he embarks upon a highly positive and universal proposition. He says that we are all ignorant and must ever remain so; and it is here that I join issue sharply with him. But before dealing with the details of his paper, I wish to make a few criticisms on the general outlook of scepticism at large.

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The agnostic attitude was first brought into popularity in the course of last century, and mainly through the agency of Huxley. By him it was applied solely with regard to questions of metaphysical or ontological character. He found in existence all sorts of ridiculous "explanations" as to the origin of the universe, and he pointed out that such questions are entirely beyond our knowledge. An immense vogue was subsequently acquired by agnosticism, and a tendency to apply this attitude all round, in spheres very different from what Huxley intended it for. As in all cases of rather sudden mental conversions, the new belief is held in too dogmatic and universal a form. The exaggerated claims of the sphere of knowledge were succeeded by inadequate claims, and for a long time past, many persons have been attempting to invest somewhat foolish opinions with an appearance of scientific authority, by loudly protesting the agnostic attitude.

Now, the fact is that, except in those metaphysical region stigmatised by Huxley, the agnostic attitude is *not* one that is often appropriate in scientific questions. For scientific questions, unlike metaphysical questions, are theoretically soluble, even if they are not practically so at the moment. It is true that on very many disputed scientific questions the evidence is indecisive: we do not know the correct answer to such questions, but we have little doubt that our descendants will know them. These matters are not beyond the sphere of knowledge, they are merely dependent on the presence of a sufficiency of evidence. On the present controversy of Vitalism *versus* Mechanism, Huxley himself was far removed from agnosticism: he defended the materialist view with his characteristic vigour and directness.

Science, unlike metaphysics, does not lead always into deeper doubts, but on the contrary leads to a large number of very positive and definite beliefs, and of equally positive and definite disbeliefs. That, in short, is the value of science, that is the cause of its progress in contrast with the stagnation of metaphysics. Science builds upon a rock of proved stability. Were there any room for doubt or scepticism, it could not build, for the superstructure would crumble down as rapidly as it was built up—even as we see in metaphysics.

Now, like many of the words used in philosophical disputation,

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the word "agnosticism" is very vague in its meaning. It may mean particularly either of two very different things. The agnostic answer to a question is effectively to say that we do not know the answer: but our grounds for affirming ignorance may be either that the evidence hitherto adduced is inadequate for forming an opinion, or that the whole subject is beyond the range of human knowledge, and not amenable to any evidence either now or in the future.

In its first acceptation, agnosticism must of course be universally present in scientific investigations. Everyone commencing a research starts as an agnostic with regard to the question he seeks to solve; for if he knew the solution of it, he would not need to conduct an inquiry to ascertain it. Agnosticism has a more vital signification when it connotes not merely present and accidental ignorance, but permanent and necessary ignorance, due to the subject of inquiry being outside the range of scientific method, and hence outside the range of human knowledge.

I revert now for a moment to the first type of agnosticism:—that in which there is a mere expression of ignorance, which is liable in course of time to be removed. What should be our attitude with reference to problems in which there is no actual inductive evidence at hand? It is often said that we can form no opinion; but as a matter of fact we can very often fall back on *à priori* assumptions of such strength as to leave very little to be desired in furnishing *data* for an opinion. Let me take examples.

Not long ago, I heard a lady discoursing on the beauty of the full moon which she had seen setting at ten o'clock in the evening, a few weeks previously. Now *à priori* reasoning at once suggests that the full moon, being on the opposite side of the earth from the sun, must follow the sun's diurnal motion at about twelve hours' interval: and hence that if the lady's statement were veracious, the sun must have set about ten o'clock that morning (for it was the season of the equinox). Accordingly, I questioned the accuracy of the observation: and it was promptly confirmed by a gentleman who said he had seen it at the same time. Here then were too independent items of inductive evidence, bearing each other out, not opposed by any direct evidence whatever, but only by an *à priori* argument. Both the witnesses were absolutely honest, both more-

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over were highly intelligent, both were absolutely convinced of the truth of what they said, and both agreed as to the exact point behind a tree on the western horizon, at which the full moon was setting at 10 p.m. At the date in question I was not thinking of the moon, nor was I in that part of the country : hence there was no antagonistic evidence.

Yet the combined affirmations of these two observers weighed as nothing, in comparison with the *à priori* assumption against them. That *à priori* assumption is of such force, that the contrary testimony of a hundred persons would not suffice even to weaken it. The *à priori* argument carries greater conviction than any amount of ordinary testimony.

Now let me apply these considerations to the class of questions, on which Sir Oliver Lodge urges us to keep "an open mind" :—that is, to maintain the agnostic position. He affirms that certain kinds of spiritual occurrences take place under certain conditions. He cannot reproduce those occurrences, for the conviction of impartial persons : and evidence which cannot be publicly examined is of course scientifically worthless, as Sir Oliver must no doubt agree. Hence we are in the position of having no definite evidence : and we are asked by Sir Oliver to keep an open mind on the question of the actuality of the alleged occurrences. But surely he could never ask us to assume the agnostic attitude, if he had reflected for a moment on the *à priori* probabilities at issue. Spiritually-caused phenomena are not within the experience of any living being. In the entire sum of human knowledge, the record is unbroken that all events are materially-caused. Not one single instance is known of any event having ever been produced at any time by such an agency as that indicated by Sir Oliver Lodge. Yet for innumerable centuries the highest intelligence of man has diligently sought for evidence of that agency : still more, the emotions and prejudices of great and small have been enlisted with almost absolute uniformity in making out a case for spiritual activities. How does Sir Oliver dare ask us to keep an open mind on this question ? The probabilities against his view are far more overwhelming than in my modest example of the moon. For, after all, the time of setting of the full moon is deduced from the theories of the solar system : and these theories rest upon observations, vastly numerous,

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no doubt, but still confined more or less to a specialised section of interested observers. But the material origination of external events rests upon the observation, not of a limited class of observers, but upon all mankind at every moment in their lives. Their deepest prejudices incline them to the spiritual view, their leaders of thought have throughout history assailed the problem: *and not one single instance have they found.* In the face of this, we are now asked to keep an open mind, on the strength of a certain number of "observations" by superstitious gentlemen who cannot place their evidence before the public. Was ever so preposterous a demand made upon our patience or our intelligence! The only attitude that bears the slightest atom of reason is an attitude of intense and unmitigated disbelief in all those spiritual manifestations which Sir Oliver believes himself to have witnessed. We should flatly disbelieve them: we should keep our minds tightly and consistently closed against any sort of appeal that he may make in their favour.

As Sir Oliver says in his address: "Let those who prefer the materialistic hypothesis develop their thesis as far as they can; but let us try what we can do in the Psychical region, and see which wins." Seeing that the entire sum of modern knowledge rests upon the materialistic hypothesis, and seeing that the "psychical region" has not yet given rise to a solitary item of knowledge of any kind, the parallel would appear to be slightly one-sided, and convey no very striking idea of the speaker's sense of proportion. But by all means let him do what he can in the psychical region. But why talk so big about it, before doing anything? So far it is nothing but talk: a mountain of words from which never so much as a mouse emerges. I cannot refrain from one or two further criticisms on this address. "Biologists in their proper field are splendid," says Sir Oliver. Yet notwithstanding their radiant qualities, they no sooner reach a conclusion which offends his spiritualistic prejudices than he says, "appeal must be made to twelve average men, unsophisticated by special studies." And I wish to ask, on such a question as free-will, what ghost of a chance have twelve average men of expressing any opinion of the very slightest value? What did average men think of Copernicus, of Galileo, of Bacon, of Spinoza, of Priestley, of Darwin? If Sir Oliver

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Lodge fell ill, how would he like to be doctored by twelve average men, unsophisticated by special studies ? What moreover is meant by an average man ? For opinions differ greatly between the different social classes, different localities, towns, countries, etc. Twelve average men would on a scientific question usually give an opinion entirely at hazard, depending on their education or extraction. For the only attribute which Sir Oliver insists upon their all having in common, is unsophistication—that is to say, ignorance—on the subject on which they are invited to adjudicate. I confess that Sir Oliver's opinions are much of the kind that might be expected from the combined efforts of twelve average men ; and if so they furnish a lasting condemnation of the attempt to set up the ignorant prejudices of mobs as the standard of philosophic truth.

And what arguments does Sir Oliver use in his defence of Vitalism ? He dashes in with a peacock's tail and says it is " exceedingly difficult to explain " how the pattern came about by mechanical means. The old worn-out solitary argument of Vitalism, without even an attempt at disguise ! Weary of repetition as I am, I must yet allude to this argument. It reduces itself to this : here is a thing I cannot explain mechanically : therefore there can be no mechanical explanation : therefore mechanism is false : therefore Vitalism is true. It is just like Driesch, only far balder and more crude. Of course a peacock's tail is difficult to explain : but no one has ever alleged that mechanism furnishes all things with a prompt and obvious explanation. That Sir Oliver Lodge should say that mechanism is false because he cannot understand a peacock's tail is to arrogate to himself an understanding of Nature which is infinitely beyond the powers of any man. However, if he insists on this argument, he will at all events permit his adversaries to resort to similar logic. I cannot understand how God made the universe : therefore he did not make the universe : therefore spiritualism is false : therefore materialism is true. How very simple it all is, to be sure ! But perhaps Sir Oliver *does* understand how God made the universe.

I observe that consistency is not a striking characteristic of Sir Oliver Lodge's opinions. He says that " if we say we can reduce everything to physics and chemistry, we gibbet ourselves as

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ludicrously narrow pedants, and are falling far short of the richness and fulness of our human birthright." What our human birthright has got to do with it, I really cannot imagine. What I do comprehend however is that Sir Oliver describes his adversaries as ludicrously narrow pedants : and I then travel laboriously through his next two pages to where he advocates that both materialists and psychists should develop their thesis as far as they can, and "neither should abuse the other for making the attempt."

Lastly, I must quote the last sentence of Sir Oliver's address : "We are deaf and blind to the Immanent Grandeur, unless we have insight enough to recognise in the woven fabric of existence flowing steadily from the loom in an infinite progress towards perfection, the ever-growing garment of a transcendent God." Now really, what does this all mean ? It is vague and abstract to the last degree. Must we leave it vague and abstract, or is it permissible to analyse it, to try to get some sharp and concrete meaning out of it ? That is the only course, unless, as I suspect, it is a mere string of words.

Well then, "the woven fabric of existence." Existence is made analogous to a cloth issuing from a loom. Before existence, there must have been a loom, an infinite quantity of yarn, and power for working the loom. The yarn is the raw material of the cloth—*alias* woven fabric—*alias* existence. The raw material of existence must therefore have been in existence before existence itself. It flows "in an infinite progress towards perfection" : that is to say, the character of the cloth is improving as time goes on, and this improvement will continue *ad infinitum*. Whether it will ever reach perfection we are not told : at present all we are informed is that the cloth is of imperfect manufacture, though better than it was once. Finally, it is "the ever-growing garment of a transcendent God" ; from which I gather that Sir Oliver's god requires clothing, that he uses for the purpose the inferior brand of cloth called existence, and moreover that he wears more clothing every day. His garment is "ever-growing." As the cloth improves in quality he takes more of it instead of less as a human being would do. In fact he is apparently obliged to wear the entire output of the loom.

Now if Sir Oliver Lodge's sentence means anything at all, it means that. It may well secure the suffrages of twelve average

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men, unsophisticated by special studies : but the respect of thinkers, it can never secure.*

It is from considerations of the above character that I long ago closed my mind to the reception of evidence of the kind sought by the Society for Psychical Research. It appears to me of the highest certainty that no such evidence ever will or can be found. That a little body of prejudiced investigators should be deceived, appears to me so natural and probable a phenomenon, as to invite no interest or comment. That the phenomena they believe in actually occur, while never having been discovered by science or commerce, or recorded in any trustworthy history, appears to me of the last improbability. This must be my justification for the "supercilious tone" for which Dr. McDougall rebukes me in dealing with the S.P.R. (Society for Psychical Research).

This society, which I take to be a sort of Home for lost and starving Ghosts, is, I believe, patronised by a large number of well-meaning if "somewhat obtuse and disingenuous" persons. I am not aware whether the society is affiliated with the corresponding Battersea Home for Cats ; but I think this latter institution might furnish several suggestions of value to its sister-society. For the kindest thing you can do to a ghost is to put it in the lethal chamber and then bury it in the Society's cemetery, unless that should already be overcrowded with the defunct relics of the Society's once-vaunted beliefs.

* Rhetorical effects are generally most pronounced when the meaning of a passage is sufficiently shallow to detract the listener's attention to no serious extent from the words employed. It is then only necessary to throw together a number of words, each of which has a certain emotional content. The association of these words has a cumulative effect in raising emotion in the audience ; so that, although the emotional colouring of each individual word may be slight, the summation effect of them all is considerable. In the short sentence above examined, there occur such words as immanent, grandeur, woven fabric, infinite, progress, perfection, loom, garment, ever-flowing, transcendent, God. In order to get clear of oratorical effects, we have to substitute unemotional synonyms for these emotion-raising terms, and we have at the same time to banish their vagueness by substitution of precise and concrete conceptions. Take, for instance, "the ever-growing garment of a transcendent God," and endeavour to give the word "garment" a sharp and concrete meaning, free from emotional content. Shall we say waistcoat, or dressing-gown, or hat ? The absurdity is increased if we endeavour to particularise still further. Do we mean tall hat, or bowler hat,

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I think I hear Dr. Mercier, with his cocks and hens, talking about the undue force of my expressions. So be it : I fear it is not in me to be milder ; those who really have in their hearts the welfare of science will not think my words too strong. Hitherto I have scarcely touched upon Dr. Mercier's article, and I now turn to give it the attention which is necessarily aroused by any writings of this distinguished author. Although it is now my misfortune to be in the opposite camp to him, I am yet indebted to his various writings for much instruction on many subjects. If he had never done anything else, Dr. Mercier's review of Karl Pearson's *Grammar of Science* in a former number of BEDROCK would of itself stand as permanently valuable, for he there succeeded in refuting in the most unanswerable manner the alleged fundamental distinction between the "how" and the "why." Moreover I have never observed in any of Dr. Mercier's numerous controversies that he is at all backward in the use of vigorous terminology : if he persists in throwing stones at me he will only damage his own residence. This being so, I think I need make no apology to Dr. Mercier for the refutation which I am about to offer of his views. Dr. Mercier's attitude is the attitude of scepticism, which I have endeavoured to stigmatise as weak and heuristically barren. But he carries this scepticism to an unusually extreme degree. In fact, he seems rapidly to be approaching the ideal of believing in no positive doctrine whatever—except, indeed, the inheritance of acquired characters.

In October, 1912, I summarised the arguments against Vitalism in a series of twelve propositions, eleven of which I alleged to be non-contentious. Dr. McDougall has maintained an ominous silence on this matter ; but Dr. Mercier now takes up the challenge,

or white helmet ? The use of general terms is only justifiable when we wish our statement to cover a large number of individual concrete cases : it is not justifiable for the purposes of vagueness and emotionalism. Sir Oliver Lodge's general terms cannot be made to cover any concrete individual case without being reduced to absurdity. It does work, however, in the minds of unsophisticated men, an emotional effect of more or less agreeable character. When the speaker, after raising such an effect, suddenly resumes his seat, the audience is left in a condition of emotional strain which (as in all such cases) finds relief in some active manifestations. The ordinary manifestations are in the production of noise : and where the emotion has been agreeable the noise is canalised into the conventional form of cheering.

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and proceeds to contest four of my propositions. My propositions (5) and (6) ran as follows :—

“(5) As in the past, the sole evidence offered for them [animate causes] is that mechanical explanations have not yet been proved.”

“(6) No direct evidence of any kind has ever been found for the existence of a vital force.”

Dr. Mercier says that :

“This is clearly erroneous,” for “every exertion of the will that is followed by a bodily act or movement is evidence for the existence of a vital force. I do not say that it is irrefragable proof ; but incontestably it is evidence, and evidence that cannot lightly be dismissed. Indeed, I think it is quite fair to say that it is sufficiently cogent evidence to enable the hypothesis of Vitalism to hold the field in this particular region until the hypothesis is disproved. . . . To the unsophisticated mind nothing appears more certain than that the mental operation of the will is the cause of the material movement.”

What is there about the unsophisticated mind that has such charms for vitalistic writers ? Are the ultimate problems of physiology to be referred for arbitration to a jury of rustics and ploughmen ? For these, at all events, are unsophisticated. To such people, as Dr. Mercier says, “nothing appears more certain” than that a spiritual will causes by itself a material movement. The instance is singularly unfortunate, for nothing *is* more certain than that a spiritual will does not by itself cause a material movement. Few people know as well as Dr. Mercier that a material movement is caused by a material neurosis in the brain. That no vitalist would venture to deny : the most that they affirm is that the will may act as a directive spark guiding the neurosis into one or another channel. There is not a single physiologist living who imagines, as unsophisticated persons imagine, that muscular action follows immediately upon the operation of a spiritual will. They know that muscles are excited only through the agency of a nervous apparatus, of which the unsophisticated person knows nothing.

My statement was that no positive evidence of any kind can be found in favour of the vital force. In his endeavour to controvert that statement, and to find some positive evidence, Dr. Mercier drags in the unsophisticated mind, which is known and admitted

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by all sides to be wrong. I cannot admit as evidence in favour of Vitalism, the admittedly false superstitions of uncultured people. The fact that Dr. Mercier is driven to this extremity in the attempt to find evidence for Vitalism seems to me to justify more than ever my statement that it is impossible to find any evidence of any kind in support of Vitalism.

On this question, I wish to raise a point of logic for Dr. Mercier's consideration on anti-Aristotelian lines. Dr. Mercier affirms at the beginning of his article that the problem of Vitalism is "completely insoluble." By that I understood him to mean that there is no evidence available on the matter. He then places the unsophisticated mind in the witness-box, and obtains from it what he is pleased to regard as evidence in favour of Vitalism. Now, can a problem be rightly posited as insoluble, if there is actually a certain amount of evidence at hand? That evidence may certainly not be sufficient to constitute proof. But any evidence sets up a probability in favour of one side or the other: and so long as there is any evidence, however slight, we cannot truly say that we are in complete ignorance on the matter: for there is a greater or smaller presumption, which might indeed be mathematically estimated, in favour of that side to which the evidence points. A further fact arises also: if *some* evidence exists, that is clear proof that the problem is not beyond the range of human enquiry by inductive methods. Where there is already some, there may hereafter be more: there may be enough to solve the problem altogether. In short, the presence of evidence suffices to remove the problem altogether out of the sphere of agnosticism: a term originally and rightly applied to those metaphysical regions in which there neither is nor ever can be any particle of evidence whatever, on account of the nature of the problem itself. It seems to me, therefore, that Dr. Mercier is scarcely consistent in first calling the problem insoluble, and then adducing what he deems to be evidence in favour of a particular solution.

The other two of my propositions contested by Dr. Mercier are:—

"(8) The whole nervous system is built up on the reflex principle, and forcibly suggests mechanism."

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“(10) Vitalism involves a creation of energy or of matter. It is proved that neither takes place.”

Now, proposition (8) is one of the received doctrines of modern physiology. It is stated and substantiated in the most complete and unquestionable way by Professor Sherrington in his *Integrative Action of the Nervous System*. It is accepted by Dr. McDougall in his *Body and Mind*. Dr. Mercier is entirely singular in his refusal to accept it: this he frankly admits himself: he says he knows “quite well that all nervous action is generally supposed to be either directly or indirectly reflex,” and he combats this doctrine, as he says, “speaking for myself alone.” I fear I cannot follow the discussion here. Writers on the philosophic aspects of a science are bound to accept the facts of that science, as stated by the acknowledged authorities. It is enough for me that my proposition is based upon the highest and widest authority available. That Dr. Mercier has to bolster up his theory with a very heterodox view of the facts is a strong presumption against the theory.

The same observation applies with even greater force to Dr. Mercier's objection to my statement that “it is proved that no creation of matter or energy ever takes place.” He replies:—

“What possible evidence could be given that at no time in the long stretch of eternity, and at no place in the infinite dimensions of space, had a particle of matter or a spark of energy ever come into existence? Again, Mr. Elliot has assumed hastily and inconsiderately the *onus probandi*. I do not assert that energy and matter can be, or have been, created. I make no assertion at all either way.”

Here is an excellent instance of scepticism run mad. Dr. Mercier may make no assertion either way, but he passes the whole of his existence on the assumption that matter and energy will not spontaneously come into existence out of nothing. But again, the point is not one I need argue. I simply take the facts as represented by the entire body of physicists. The conservation of matter and energy is the thickest pillar of physics and chemistry. The whole of engineering and allied sciences rest upon it. It is one of the most certain of all items of human knowledge.

Dr. Mercier asks how we can know that there was not some period in past history when it did not hold true. That question is

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sound : I agree that no evidence can be offered to prove that it always and inevitably has held true. But Dr. Mercier should not bring his criticism to bear on a single physical principle such as the present : he should bring it to bear on the validity of human knowledge in general. How can we prove that there may not be in the infinite depths of space a star in which gravitation is occasionally reversed, so that bodies fall upwards into the sky ? How do we know that there may not be a star where fire burns up iron and melts wood ; where the rivers flow vertically upwards ; where water boils at low temperature and freezes at high ; where whales perch among the tree-tops ; where eels stand up on the tips of their tails and dance over the face of the land, singing ragtime. I am not in a position to offer any evidence against the possibility of any of these eventualities. We human beings cannot go beyond what we see and experience. Our experience teaches us that this is an orderly universe : and as we increase our knowledge we find a certain number of laws to which no exceptions have ever been observed. One of these laws, and one of the most striking manifestations of the orderliness of our universe, is the law of the uncreatability of matter and energy. It may not have an infinite and absolute validity ; but its validity at all events is as high as any that can be attained by human knowledge. And that is all I care about. It is all, I think, that anyone need care about : certainly all that anyone can ever know.

Up to date, then, Dr. Mercier's arguments amount to this. Firstly, as direct evidence of Vitalism, he adduces the admittedly false opinions of the "unsophisticated mind." Secondly, in order to assist his indirect attack on Mechanism, he is led to deny (a) the results of modern physiology as to the constitution of the nervous system ; (b) the best-established of all the principles of physics. If we base our practical life, as we all do, and our theoretical philosophy, on the assumption of the truth of the main laws of science ; then equally we may base our practical life and our theoretical philosophy on the assumption of the falseness of Vitalism.

One or two minor points still remain to be answered from Dr. Mercier's article. He refers to the possibility of physical energy being transformed into "latent will" ; and apparently thinks it is a new theory which will make my "flesh creep." I fear I crept

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not in the least, having been familiar with this theory ever since I read Butler's *Analogy* at the age of seventeen. It is what I have often called crude materialism ; and was widely held before the introduction of the modern scientific materialism. It has only two objections : (1) that no item of evidence has ever been found for it ; (2) that the general structure of the nervous system is such as to offer the most insuperable difficulties in the way of any such hypothesis. But why discuss these hypotheses and theories ? If a hypothesis of so venerable antiquity has never in all these years found a single fact to support it, why discuss it ? It is a mere guess among a thousand others as to the origin of mind, and no guess is of the very slightest vestige of use. It is worth just about as much as the innumerable other speculations, with which the unsophisticated mind is accustomed to amuse and astonish itself.

Dr. Mercier further contests my proposition that if $a = b$, then $b = a$: which, he affirms to be true in mathematics, but false in logic. The matter is merely one of the use of terms. By the sign $=$, I intended to express the relation of identity : whereas Dr. Mercier interpreted it to imply that a was only a special case of b . What I meant to say was that if, as idealists affirm, the substance out of which matter is made is the same as that out of which mind is made, then it follows that the substance out of which mind is made is the same as that out of which matter is made. Dr. Mercier probably did not recognise that I was endeavouring to establish so simple a truism. But is it not often the simplest things that are most difficult to understand ? At all events, I thank Dr. Mercier for his criticism, and shall endeavour to avoid in future this possible loophole for misinterpretation.

Finally, Dr. Mercier cites my reference to " those melodramatic and thaumaturgic instincts, which pullulate in every public assembly, and in every newspaper " : and he goes on to say that he shrinks from such horrors, especially as he has not the slightest notion what I mean by them. He assures me that he is innocent of melodramatic and thaumaturgic instincts, whatever they may be. If Dr. Mercier really does not know what these words mean, I submit that a handy way of finding out, would be to look them up in a dictionary. " I swear I do not pullulate," says Dr. Mercier, " nor do my instincts pullulate in me." Now, I may inform Dr.

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Mercier that the word "pullulate" means to germinate or bud forth, and is widely used to indicate active and creative production of new ideas. I trust Dr. Mercier will be reassured when I admit, as I freely do, that I discern no tendency whatever to pullulation in any part of his attack upon myself.

THE NEBULAR HYPOTHESIS AND ITS DEVELOPMENTS—III.

By Professor H. H. Turner, F.R.S.

IN the first article of this series (BEDROCK, II., 1, p. 67) I quoted some words of that great thinker, Henri Poincaré, to the effect that the hypothesis of Laplace, while it fitted the facts of our own system better than any other, could at best be only a particular case of a more general hypothesis, seeing that there is great variety among stellar systems. For instance, our system is dominated by a single sun ; we know very many systems where there are two suns, and many again where there are three. It is practically certain that if there are subsidiary bodies in these systems they must not only behave to-day in a manner very different from our planets, but must in ages past have had a very different manner of birth. Our planets pursue nearly circular paths round their master the Sun ; but how shall a planet serve two masters ? It taxes our resources to give even an approximate answer to the question, and the path indicated is very far from circular. It may for instance, be a figure of eight, with one Sun in each loop ; and it is conceivable that there might be an analogy to Saturn's ring in the form of a figure-of-eight, and that this ring might condense into a planet that continued to move in the orbit over which it was formerly spread. There would thus be a certain resemblance between the genesis of an attendant on the twin suns and that of a satellite of our Sun : but even if we grant this very unlikely supposition, it is obvious that we have developed our previous conception and rendered it more general.

Moreover, how did the twin suns originate ? At first sight it might seem that one could originate from the other just as a planet

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from our Sun, the division into two nearly equal pieces instead of a large and a small one being accidental. But we saw in the first article that it is of the essence of the matter that the Sun should be large and the planet small. Indeed, the planet is not formed from the Sun himself, but from his tenuous atmosphere (p. 71), and we cannot indifferently assign the different parts of Sun and atmosphere to two nearly similar bodies without absurdity.

The method of formation of twin suns has received much attention, and the accepted solution is that a rotating single body, contracting under gravitation, and thus rotating more quickly, at a certain point in its history loses its symmetrical form (round the axis of rotation) and becomes pear-shaped: that the neck of the pear contracts, becomes so small that the body resembles an hour-glass, and finally breaks, so that two bodies revolving round each other take the place of the single rotating body. Each will cause "tides" in the other; and tidal action will drive the bodies apart. We can scarcely stop to follow the process here; it has been ably described, in his popular book on "Tides," by the late Sir G. H. Darwin, who first worked it out in detail for the case of the Earth and Moon. This particular example is, however, of profound interest for us, not only because we happen to live on one of the bodies concerned, but because it is almost certainly unique in the Solar system.

All our planets, including our Earth, were probably generated in the manner described in the first article: not from the Sun himself, but from his "atmosphere" or envelope—perhaps the term "envelope" is preferable, as atmosphere has a restricted modern sense. All their satellites, with the single exception of our Moon, were probably generated in the same manner from the envelopes of their primaries. But our Moon was separated, not from the envelope of our Earth, but from its actual body, as one Sun is separated from another. There is, in this case, still a considerable disparity in size, seeing that the Earth would make eighty-one Moons: but the disparity is much less than in the cases of other planets and their satellites. Saturn is nearly 5,000 times the size of his largest satellite, and Jupiter more than 10,000 times.

Why there should be this single exception to a rule which has been followed so uniformly through the rest of the system is not yet clear: but the reason must lie in some variation of the initial

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conditions. And we are thus brought back to Poincaré's remark that Laplace's hypothesis can only be a special case. It suits the Solar system very well, but even within that system we find a complete exception. It does not require much imagination to infer that when certain conditions are further varied (though we may not be able to specify the conditions or the variations) there will be further notable exceptions.

Enough has perhaps been said to show why, without disparaging Laplace's hypothesis in its applicability to certain cases, we should nevertheless look beyond it for the elucidation of others. It may be the truth, but it is certainly not the whole truth. There is, moreover, one further point of considerable importance in this connection. In following the genesis of a planet from a central Sun, we *started* (p. 71) with "a central nucleus or Sun, surrounded by a diffuse and tenuous atmosphere." Now, though this is the point from which we took up the history, there must obviously be an antecedent history which was taken for granted. How did the original nebula or chaos condense into this particular form of nucleus and envelope rather than another? Why, for instance, were there not two nuclei? This is not quite the same problem as that of the twin suns above sketched; we there supposed the nucleus to divide after it had become compact: but clearly we could put the division much earlier, and though the difference is in one sense only a difference of degree, we can never be sure that a difference of kind will not result. Our Earth only differs in degree from Mars, and yet produced its satellite in an entirely different way.

It is the purpose of this article to consider another possible process of development, differing very greatly from both that suggested by Laplace for the Solar system, or that worked out in recent times for double stars and the exceptional case of the Earth-Moon. It is as yet little more than a speculation, but it is a speculation suggested by observed facts; it is the simplest hypothesis which will fit them; and finally, there is at the time of writing, no known fact which cannot be reconciled with it. Without these three characters it would scarcely merit attention; with them it may claim consideration until either a better hypothesis is framed or some clearly contradictory fact is encountered.

It will have been noticed that both the methods of genesis

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previously mentioned have been directly suggested by observations. Neither Laplace nor his predecessors might have thought of the first but for the spectacle of Saturn's rings, and the consistent whirling of our System all in one direction. Laplace thought backwards from these facts to his hypothesis. Even more directly Sir G. H. Darwin worked backwards from the existing state of things, when the Moon is far distant from the Earth, to the time when she was nearer and nearer and ultimately in contact. He followed the changes in length of the month and day by mathematical analysis, finding to his delight that they came into coincidence, as they should, when Earth and Moon were in contact, practically forming one body. It is incumbent on us also to work backwards from existing facts, and we must therefore first recall those which are important for our purpose. They are chiefly two, and they have already been detailed in the first number of this Review ("The Stars in Their Courses," BEDROCK, Vol. I., pp. 100—107). The first is the apparent bifurcation of the universe, and the second is the increasing velocity of a star with age: the first is a recent revelation of the older astronomy which studied the positions of the stars in the sky; the second a triumph of the newer science which dates from the utilisation of the spectroscope: and though each in turn has profoundly modified our views of the constitution and history of the universe, ten years ago there was not a suspicion of either.

It was in 1904, at the Congress of Arts and Sciences at St. Louis, that Kapteyn drew public attention to the first great fact. The method of its recognition can be concisely stated in terms of an apparent inconsistency. It has long been known that our Sun is in movement among the other stars, because we can recognise its line of advance by seeing the stars opening out in front of us, flying past on our flanks, and closing in behind, much as we see the scenery behave in a railway journey. There is, however, this essential difference in the two cases, that the scenery in the terrestrial journey remains at rest (one part relatively to another) while the stars through which we wing our heavenly flight are all in movement just as we are ourselves. The phenomena will, however, retain a certain resemblance if the star movements are "at random"—indifferently in all directions; they will still open out in the direction of advance and close in towards the direction of retreat, and by

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careful measurements we can accordingly identify both these directions, which should, of course, be opposite to one another. And now comes the inconsistency. The two directions have been separately identified, and are distinctly *not* opposite to one another. For simplicity we have mentioned these two directions only; but the flank movements of the stars are also available as evidence, and their testimony supports the former. Many independent investigations have been made since Kapteyn's announcement in 1904 without shaking the central fact of inconsistency.

What, then, is the reason of this inconsistency? We need go no further than the supposition above made that the star movements are at random: if they are not at random, there is not longer any necessity that the direction identified as that of advance should be opposite that of retreat. To take a simple example: suppose a spectator standing on Salisbury Plain sees a body of troops advancing on him from the south: the solid ground beneath his feet tells him that he is at rest and the troops in motion: but in space, having no corresponding guide, we should attribute the movement to ourselves, from sheer modesty, identifying our line of advance accordingly as southwards. But suppose there was also another body of troops on the Plain receding from the spectator towards the east: he would identify his own line of retreat as westwards. Clearly he could not be advancing due south, and retreating due west at one and the same time: he would infer that his modesty in attributing both movements to himself had been ill-timed; the troops cannot both be at rest; one at least must be in motion, and without further enquiry we could say that each was in motion *relatively to the other*.

The celestial inconsistency leads us to a similar conclusion: it is possible to divide those stars whose movements have been observed (a mere handful of the total number visible in our telescopes, but nevertheless including a good many thousands) roughly into two groups, one of which is in movement relatively to the other. It is further possible to specify the direction of relative motion, and this has been done by various independent investigators with satisfactory agreement. One possible interpretation (put forward in illustration merely) would thus be as follows: Two great clusters of stars, formerly separated in space, have met and completely

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intermingled. The distances between the members of each are so great that there is small chance of a star of one cluster hitting one of the other : the clusters are penetrating one another as the assemblage of ships crossing the Atlantic from American ports to European penetrates the assemblage crossing from Europe to America, the courses being only roughly parallel, but with average tendencies in opposite directions : and it is conceivable that one cluster will ultimately pass clean through the other so that they will be again separated, though in the opposite direction from at first. Now such a manœuvre would suit the facts as we know them ; but it expresses a great deal more than we can answer for. All we know applies to a mere moment in the history, when the clusters are thoroughly mixed. The former separation and the future separation are mere accessories introduced to give a mental image of the present state of things. (It may be, for instance, that one cluster as a whole attracts the other as a whole so strongly as to arrest its movement before it gets clear, and a return movement is then set up, which is again arrested before clearance on the other side.) Further, the very nature of the motion is only partially known, since the observations from which it is inferred are only partial. When we watch a star with the naked telescope, and see it move in the heavens, it is but one part of its movement that we see, viz., that across the line of sight : and unless we can (independently) determine the distance of the star, we cannot interpret the movement in miles per second. For the vast majority of the stars no such determination of distance has been made : and the conclusions as to their systematic motions are thus subject to fundamental limitations. The chief limitation may be illustrated by another terrestrial example. Standing between railway lines, we see them converge to a point in the distance, according to the well-known perspective principle. We know that they are really parallel and that the convergence is only apparent : but if they did actually converge, they would still seem to converge. If we could divest ourselves of the knowledge that the lines were parallel, we should not know whether the convergence was real or apparent. We might find out by measurement : for instance, if we knew that a certain point in a rail was 1,000 feet away, we could convert the angular separation at that point into actual feet, for comparison

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with the separation at our standpoint: and then if the two were equal we should infer that the rails were parallel. But in default of this knowledge of *distance*, the separation at the inaccessible point would not be determinable.

It is this lack of knowledge of *distance* which similarly limits our interpretation of stellar movements, so that we cannot say whether they are parallel or actually convergent. We know the distances of a few of the nearest stars, and something about that of others can be inferred by various indirect processes. Wherever such knowledge is available, it helps materially in the interpretation; but it is as yet too scanty and uncertain to distinguish positively between parallelism and convergence. In the above terrestrial example the knowledge of a single distance would suffice; but the scenery is in that case at rest. The stars are all in movement and we can only deal with average motions from which any individual (where we may be fortunate enough to know the distance) may differ widely.

Before proceeding to consider the second great discovery of recent years we will now outline generally the manner in which the first may be explained, and we may profitably commence by an illustration from our Solar system. We know that, over and above the planets which are circulating round the Sun at nearly constant distances from him, there are numerous comets which behave in a very different manner. Their orbits are long, thin ellipses, of which one end loops round the Sun. A comet spends most of its life at the other extremity, loitering slowly round the bend under the detaining backward pull of the Sun. Once round the bend, however, the Sun's pull accelerates the motion, which becomes more and more rapid until there is a mad rush round the bend near the Sun and a skurry away to the depths of space again. We only see a comet when it is near enough to the Sun to be illuminated by him sufficiently: but suppose a superhuman observer could take stock of all the comets in our system. He would see about half of them on their way to the Sun, and about half on their way out again from the Sun. Their paths are curved, but not much curved; if we could deprive them of the little curvature they have, then half the comets would be headed straight for the Sun, the other half straight away from him: so that the superhuman observer, taking stock of their movements at any given moment,

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would find half of them converging to a point and the other half diverging from it. Let us now blot out the Sun from his vision—instead of the Sun being a visible centre of attraction, let it be an invisible centre. The convergence and divergence would remain, and the superhuman observer, knowing that this was a possible perspective effect of parallel motions, might infer that there were two groups of comets moving in opposite *and parallel* directions just as it has been inferred that there are two groups of stars moving in opposite *and parallel* directions, the words italicised being in each case an assumption. If we restore the light of the Sun, the assumption would be less likely to be made: seeing a conspicuous centre of attraction towards (or from) which the groups were moving, the observer could scarcely fail to interpret the apparent convergence (or divergence) as a real one.

This illustration shows us conveniently both what we have and what we lack in dealing with stars instead of comets. We lack a conspicuous centre of attraction. Whatever else may be true, it will scarcely be maintained to-day that there is a huge central star or sun controlling the movements of other stars, as our Sun controls his planets and comets. The supposition has been made in the past, before it was realised how surpassingly great such a body must be, but our improved knowledge of the dimensions of the Universe has rendered it untenable.

In place of it another possibility has steadily advanced in favour. Suppose that the stars are collected into isolated communities,—isolation meaning that each group is separated from its neighbours by distances great compared with its own size. This supposition is as old as William Herschel, who was easily led to it from his contemplation of star clusters and nebulae, which he took to be communities outside our own. Nothing in what we have since learnt is inconsistent with that view, and on the other hand much new evidence is now available in favour of it. The chief difficulty is the increasing vastness of the conception of space, which is, after all, a psychological, not an astronomical difficulty.

Now such an isolated star-community would have a democratic government every whit as effective as the tyranny of our Sun over his system. Each member of it would have a share of power,—not a uniform share by any means, “for one star differeth from another

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star in glory": and though individual powers would often oppose one another, the sum total would be very definitely effective. Assuming for simplicity that the cluster or community is roughly spherical in shape, the resultant attraction on any individual would be towards the centre, just as though there were a central Sun: but here the analogy ends. If there were a central tyrant the attraction would increase as the individual approached the centre: with a democratic government it decreases. A star on the outside of the cluster is urged inwards by the united pull of the whole community, just as though it were all "concentrated" at the centre: but as, in response to this solicitation, it moved inwards, the attraction would diminish. Let us imagine a series of concentric spheres throughout the cluster, the outermost of which is its boundary, the innermost the centre itself. The inner spheres will contain comparatively few stars, the outermost surrounds the whole cluster. Wherever an individual may be, it will be on the surface of one of these spheres: and the law of attraction is that the individual is attracted to the centre by all the stars within this sphere. We may suppose those outside this sphere non-existent and further we may suppose all those within it to be "concentrated" at the centre. Newton proved this beautiful proposition in 1685, and he was not merely the only man at the time who could prove it, but the only man who saw the necessity of establishing it, if any real good was to come of the Law of Gravitation.

It is perhaps well to remark that though the attraction decreases as the individual approaches the centre, the velocity of its movement continues to increase until the centre is reached and passed, *i.e.*, so long as the attraction is in the same direction as the movement. When the centre is passed, the movement goes on, but the attraction is reversed and tends now to arrest the motion. In the same way a pendulum bob is continually urged to an invisible centre (represented by the position of rest or verticality) until that centre is reached and passed, when the motion is gradually arrested and reversed. The swing of a pendulum affords indeed a good representation of the way in which an individual member would move under the attraction of its community of stars. We may pull the pendulum far out to one side, in which case it will swing far out to the other: or we may disturb it only slightly, in which case its

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whole movement will remain slight. In either case the time of a swing will be the same—that is the principle of the pendulum, first realised by Galileo in the Cathedral at Pisa: whether the swing be large or small it takes place in the same time, the large swing being taken rapidly and the small one slowly. Now all these points have their analogies in the movements of the stars of an isolated spherical cluster. If a star is started far from the centre it will swing out far to the other side: if started near the centre it will only swing past it to an equally small distance on the opposite side. Were the community uniformly scattered the time of both swings would also be the same, as in the case of the pendulum. Uniform scattering is, however, highly improbable, it being much more likely that the stars thin out towards the boundary and are thicker near the centre; in which case the times of swing would not be quite the same. But they would be much more nearly equal than if we had a central Sun. The periods of our comets are notoriously unequal: some of them return every two or three years, some (like Halley's) in seventy or eighty, others not for thousands of years. Compared with this wide diversity we may regard the periods of swing in a star community as approximately the same: and at any rate those which make the largest swings must move on the whole more quickly than those which only make small swings. This notion of larger and smaller velocities characterising different stars is of fundamental importance in the consideration of the second great discovery of recent times—that the older stars move more quickly—and we proceed now to say something of the facts from which this astonishing inference has been made.

. With the unaided eye or the unaided telescope, we can only detect and measure the movements of stars across our line of vision: movement directly towards us or away from us remains undetected. But the spectroscope gives us just this lacking information, owing to the manner in which light is transmitted. It has a finite velocity (nearly 200,000 miles per second), but to this we must add the velocity of the source if it be approaching us, or subtract it if it be receding. The spectroscope enables us practically to measure the combined velocity of light and source for any given star, so that we can, by making allowance for the known velocity of light, infer that of the star. The process is a difficult and delicate one if we are to measure

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the velocities with any accuracy : Suppose, for instance, we wish to know them within a mile per second ; this is only about one-two hundred thousandth part of the whole, and the enterprise corresponds to measuring the weight of five or six tons to the nearest ounce ; or the length of three or four miles to the nearest inch. Still, it can be done, and has been done for hundreds of stars, notably at the Lick Observatory in California. The measurements are easiest when the star is bright, and for this reason it is natural to take bright stars in the first instance. But these are by no means necessarily our nearest neighbours, and it is important to notice that their proximity or distance does not affect the measures. The other component of velocity suffers the well-known diminution of distance, becoming less and less as the star is further and further away. But the spectroscopic component suffers no such diminution, and is as readily observed for a distant star as for a near one, provided the two appear equally bright.

After spending many years in organising and making such measures Professor Campbell, the Director of the Lick Observatory above mentioned, collected his results for discussion ; and there emerged from them the startling fact already noticed that the older stars were moving on the average more quickly than the younger. How do we know the age of a star ? A striking analogy that has seen much good service since the days of William Herschel is that of the trees in a forest of oaks. Without waiting to see them grow we could infer their growth by arranging them in a sequence from the sturdy giants down to the saplings, and even to the opening acorns ; and we should trust common sense to tell us that they grew upwards and not downwards. In a similar way the stars have been arranged in sequence by their spectra, and we trust common sense to tell us which is the old and which the young end of the series. When we take the *average* spectroscopic velocities of sections of this sequence we find the law above stated ; and when once Campbell had drawn attention to it for the spectroscopic velocities, Boss found that it could be extended to velocities in general.

Having thus outlined the two cardinal facts to be explained, and given a glance or two backward in the direction in which they point,

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let us boldly choose a starting point in that direction and see whether we can find our way forward to the goal presented by the cardinal facts. We will start with a large diffused spherical nebula *at rest*, and consider what happens. Laplace started with a *rotating* nebula because the general aspect of the Solar system suggested rotation. But this is clearly not so primitive an assumption as that of a nebula at rest. We can see in the sky nebulae which are apparently rotating if we may trust the indication of their spiral or whirling shape ; but we also see others, like the Owl nebula, which have a globular shape such as we postulate, and it is clearly permissible to follow out the consequences of this assumption.

The sphere will begin to contract under the gravitation of its parts, and the law of attraction has been already indicated in connection with a spherical cluster of stars. The contraction would be slow at first, but would become more and more rapid ending in an inward rush of all the parts simultaneously towards the centre. They cannot all reach the centre together, for obvious reasons. Sooner or later the concentration would become intolerable. Light and heat would be generated suddenly and fiercely, and a " new star " would appear in the heavens. There would also be reactions of an explosive character, so that the material thus concentrated would be scattered again as vigorously as it clashed together. But not in its original state of diffusion. We may confidently assume that a certain amount of cohesion originated by the enormous pressure would persist, so that what rushed together as nebula would be scattered as cohesive fragments, any one of which might ultimately form a star or system of stars. We will return to the individual fragments in a moment, but first let us continue to follow the general history. The materials which rushed together to the centre will be exploded outwards to nearly the same distances as those from which they originally came : *nearly*, because part of the energy has been dissipated in light and heat. On reaching their limiting distances, gravitation will again draw them together to a second central explosion—less violent than the first because of the aforesaid dissipation of energy : and so again to a third and a fourth. There will be recurrent explosions each less violent than the one before.

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But it is not necessary that the whole matter of the original nebula should take part in every explosion : indeed, that is very unlikely. We have supposed for simplicity that the matter was originally diffused uniformly throughout the spherical boundary and in consequence of this uniformity the approach to the centre would follow the pendulum law of being independent of the starting point ; so that all particles, whatever their starting point, would be due at the centre at the same moment. From this universal consensus comes the violence of the explosion. But this absolute uniformity is very unlikely : there would almost certainly be small departures from it : and even the smallest departure would initiate a further one, so that the exceptions would steadily grow. The general nature of the departures would be to have an excess of matter near the centre and a thinning out at the boundary : and once this heterogeneity is set up, the tendency to simultaneous arrival at the centre would be modified. The fragments thrown furthest outwards would take longest to return, so that the next explosion might have taken place before they arrived, and they might find the centre free for their own passage, might make another distant excursion, and return even more behind time for the next explosion. Thus they may survive many catastrophes, and remain as witnesses to the character of the first of all. Developing this line of thought we may see how the course of history may be changed. Not only the extreme excursionists, but others within them, may return too late for the congestion and survive as stars born at an earlier time. Meanwhile there is a second reason for diminished violence of the explosions, in that fewer and fewer fragments are in time to take part in them, and these the inner and therefore less rapidly moving. Such a description is not very clear or convincing without mathematical analysis. It is impossible to give here more than a vague general account ; but two points of detail seem to be fair inferences :

First, that the explosions would decrease in violence as time goes on, partly because of the conversion of energy into light and heat at each : and partly because the fragments no longer tend simultaneously to the centre.

Secondly, that if the explosions are the occasions of formation of stars, as above supposed, then those formed at early and more

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violent explosions will on the average have larger velocities than those formed at later and less violent explosions.

This then is put forward as a possible explanation of the second great fact to be explained—that the older stars move more quickly on the average. It will be noted that no mechanism is required beyond the known laws of gravitation and of dissipation of energy : while the only other explanations hitherto offered have gone beyond our present knowledge. Thus it has been suggested that possibly gravity does not act on matter in its early stages, but that liability to gravity is acquired. This is a speculation for which there is no warrant beyond the fact to be explained, and no further means of testing. We are bound to give the preference to an explanation which uses no new or untested physical fact, even though the realm of application be exceptional.

Returning now to the individual fragments, it is tolerably certain that they would acquire at an explosion two new characters.

Firstly, they would acquire a rotation. In describing an explosion we should almost naturally say that the fragments went “ spinning ” away to a distance ; not only is the fact of the spinning familiar to our own experience, but the explanation of it is tolerably obvious. Two fragments in contact would, for instance, tend to spin each other in opposite directions.

Secondly, though the matter may originally contract precisely towards the centre, the fragments will be unlikely to separate in directions as precisely away from it. Any little inequalities will introduce “ sideways ” movements : and in subsequent history there will be other agencies, such as occasional encounters between individual fragments, which will tend to multiply these irregularities. Thus the convergence of the fragments to the centre will lose its precise character both as regards time and space. The paths of the fragments will be *roughly* towards or from the centre, though not accurately : and as inequalities of time are set up, arrivals at the centre will be spread more and more uniformly over time, so that the tendency to simultaneous arrival which causes explosions will gradually be lost.

Looking, therefore, far ahead into the subsequent history of the nebular we *may* ultimately reach a time when—

- (1) Explosions have become rare or ceased altogether, because

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at no particular moment are there more than a fraction of the total number of stars near the central position ; and moreover their paths have sufficient clearance to avoid collisions.

(2) The fragments are survivals of different explosions. They have, therefore, had different times for individual development on their own account, and for becoming whirling Suns or systems of Suns. But their translational movements betray the date of their origin, the older moving more quickly on the average.

(3) Viewing the system at any given moment, some fragments are on their way to the centre, and others on the way out. The convergence or divergence will be rough only, but quite sufficient to respond to scrutiny.

(4) If the motions have become quite "steady," i.e., if chances of explosion have died out, there will be as many stars approaching the centre as leaving it. If the two sets are *not* equal, i.e., if there are distinctly more stars moving either in or out, this shows that the tendency to simultaneous movement has not been quite destroyed, and there is consequently still a chance of explosions.

Let us now turn to our own system of stars, which we hope is in some such stage as that indicated in No. (1), when catastrophes have become rare. To the best of our observation and belief, our own Sun and its planets have had ample time for individual development as in No. (2), and have so developed. We imagine that other stars have also followed their own development, whether like ours or not : and Campbell has ascertained for us that the oldest stars are moving most quickly, as they should. Coming to No. (3), Kapteyn has called attention to the appearance of convergence and divergence, though he did not explain it as above.

And now what of No. (4) ? Have we reached a state of "steady" motion, when we may hope our explosions are over ? The test is that the numbers of stars converging and diverging should be equal. Professor Eddington finds a ratio of 3 to 2, which is not far from equality. There is at any rate nothing to alarm us seriously, though our actually "steady state" may yet be distant.

But more detail than this may reasonably be demanded. If we belong to a star community of this kind, where is its centre, and what are its dimensions ? The direction of the centre is assigned

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by that of the converging and diverging motions but with an ambiguity: it must lie either in the constellation Taurus or in exactly the opposite direction. For two reasons my choice is for Taurus: the first because there is a congestion of stars in that direction, as there should be near the centre: the second because Mr. Lewis, by an entirely independent argument from double stars, had assigned this direction for the centre in 1906. He also assigned its distance as about ninety light years, which is supported by other evidence from stellar movements. We may accept Seeliger's estimate of some 4,000 light years for the radius of our cluster. And it is possible to calculate very roughly the oscillation period of our Sun as about 400,000,000 light years, and to estimate that we passed the centre less than 1,000,000 years ago. These figures are illustrative merely, and it would take too long to explain or defend them at the end of an already long article.

Let us rather recur to the conditions specified earlier as justification for a new hypothesis of this kind. We said (p. 475) that it should be suggested by known facts: that it should be the simplest hypothesis which will fit these facts: and that there should be no known fact not reconcilable with it.

The main facts are two: the convergence of the motions, and the increase of velocity with age. The hypothesis was directly suggested by the first of them, and was extended to include the other. As regards its simplicity it demands only (a) the isolation of a cluster; (b) the exclusion of rotation; (c) gravitation. Of these (c) may be taken for granted, and (a) will probably find few objectors. The situation with regard to (b) is curious. Had we started with a blank record it would have been more natural to leave out rotation than to put it in: but we do not start thus afresh. We have become thoroughly accustomed, through study of the Solar system, to assume rotation, so that it seems almost unnatural to leave it out. This difficulty will perhaps diminish with time. Meanwhile it may be confidently asserted that there is at present no rival hypothesis of equal simplicity. The "two star stream" hypothesis for instance contemplates two systems intermingled, without specifying whether they were formerly separated or not. If they were separated, then the isolation of either is tacitly assumed, and its own internal history is enough to provide an *appearance* of "two streams," without

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invoking the aid of another system. If, however, previous separation is ruled out, there is no mechanism for explaining the opposed motions. The hypothesis is a mere piece of description, without attempt to assign a reason : and the same may be said of "ellipsoidal" and other hypotheses.

But it is clear that only a "working hypothesis" has been provided, and that long and detailed study is necessary to clear up the many questions presented for exact solution.

A DESCRIPTION OF THE PRE-PALÆOLITHIC FLINT IMPLEMENTS OF SUFFOLK

By J. Reid Moir, F.G.S.

IN my last article in *BEDROCK* * I endeavoured to describe the geological position of the pre-palæolithic implements of Suffolk, and how they must pre-date by a considerable period the earliest Chelles, palæolithic, specimens.

I also compared the present-day opposition to these early implements with that which was at one time brought against the now universally accepted palæoliths, and this comparison showed that the method of attack of those opposed to the greater antiquity of man has varied very little, if at all, during the period of time intervening between the discoveries of M. Boucher de Perthes, and those of to-day.

There can, I think, be no doubt that if the opinions I put forward in the above-mentioned article, are found to be based on solid, incontrovertible facts, we must entirely alter our views on the question of the antiquity of the human race, and abandon the belief that the oldest palæolithic implements were fashioned by the earliest representatives of mankind.

No one, I believe, realises more than I do the far-reaching nature of these issues, or the importance of having the evidence upon which they are based subjected to the most stringent scientific criticism. But it seems to me to be necessary for those who wish to be in a position to direct such a criticism upon this matter, to be familiar, in the first place, with the fundamental subject of the fracture of

* Moir, J. Reid. "Pre-palæolithic Man," *BEDROCK*, July, 1913, pp. 165—176.

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flint, and secondly to have carefully examined the flaked flints themselves, and the deposits from which they have been derived.

It also seems to me possible that if all students of pre-history were practical flint-flakers, and had closely studied the different manners in which a flint breaks, there would not be the same wide divergence of opinion which exists to-day as to whether certain stones have been fashioned by man or the blind forces of Nature.

There are, however, a large body of scientific people who, having other subjects, to the study of which their greatest energies are directed, are not able to make themselves intimately familiar with the fracture of flint and the provenance and details of the pre-palæolithic specimens under discussion.

But, I feel it to be necessary, having described the geological horizons from which these specimens have been derived, to give to such scientific people, by means of accurate drawings, an idea of the forms of the flaked flints discovered, and thus enable them to in some measure decide whether the statements made in my last article were justifiable.

After some little amount of experience in the different methods of producing representations of these early flint implements, I have come to the conclusion that careful drawings, executed either by one familiar with the specimens to be reproduced, or by an artist under the constant supervision of such a person, are the best and most satisfactory means, at present at our disposal of conveying to the reader the general appearance of the specimens.

The drawings which accompany this article have been executed under my guidance by a most competent artist, Mr. Leonard Squirrell, of Ipswich, and give a faithful rendering of the outlines and flaking of the various types of flint implements described in my former article.

But the varying colours of the specimens due to "weathering" and contact with different staining agents, the characteristics of their surfaces and so on, are naturally not indicated in such drawings and must be described by words.

It will no doubt be remembered that in my last article I pointed out that the rostro-carinate implements from the Sub-Red-Crag "bone-bed" exhibit the large surfaces of fracture and general massiveness of form, common to specimens from that horizon.

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That those from the Middle Glacial Gravel are fashioned by the removal of smaller flakes, and altogether more graceful in their outline, while the rostro-carinates from the Chalky Boulder Clay clearly show a degeneracy in make and were evidently giving place to a more Chelles-like, palæolithic, type of implement.

On Plate I. I have figured three specimens of rostro-carinates, which are typical examples of this type from the three deposits I have described.

Plates II., III., IV. and V. show typical specimens of worked flints from the Middle Glacial Gravel, and it will be noticed that, as described in my former article, the forms and flaking of the implements differ in each group.

Group I. is supposed to be the most ancient.

Group II. less ancient, and so on.

On Plate VI. I have figured two typical specimens from the Chalky Boulder Clay which again are seen to differ from those from the earlier Middle Glacial Gravel and Red-Crag, and to approach in appearance the earliest palæolithic implements.

Though these various specimens are the most outstanding examples of the handiwork of pre-palæolithic man in Suffolk, I have discovered other series including the Strépy type of Rutot, which I shall hope to illustrate and describe later on.

My present purpose will be fulfilled if these two articles bring home to pre-historians and anthropologists the value of the evidence found in Suffolk for the existence of different races of pre-palæolithic people.

DESCRIPTION OF PLATES.

Plate I. shows rostro-carinate implements from the Sub-Red-Crag "bone-bed," the Middle Glacial Gravel, and the Chalky Boulder Clay.

It will be noticed that Fig. 1, which is from below the Red Crag, is massive in its outline, and has a large broad butt, and the implement has evidently been produced by blows of great force.

Fig. 2 from the later Middle Glacial Gravel shows the same form, only more symmetrical and graceful in its outline. The implement is much smaller than the Sub-Crag specimen and has been fashioned by the removal of smaller flakes.

Attention is also directed to the manner in which the apex of the beak overhangs.

Fig. 3 from the Chalky Boulder Clay, though still of the rostro-carinate type, is not nearly so well made, and it is apparent that the art of making these

PLATE I.

Fig. 1.

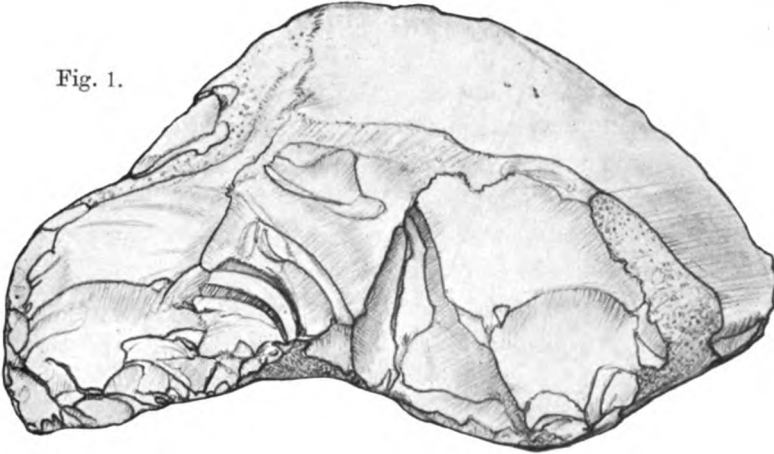


Fig. 2.



Fig. 3.



ROSTRO-CARINATE IMPLEMENTS. (Reduced $\frac{1}{4}$).

Fig. 1. —From the Sub-Red-Crag Bone-Bed.

Fig. 2. —From the Middle Glacial Gravel.

Fig. 3. —From the Chalky Boulder Clay.

PLATE II.

Fig. 1.

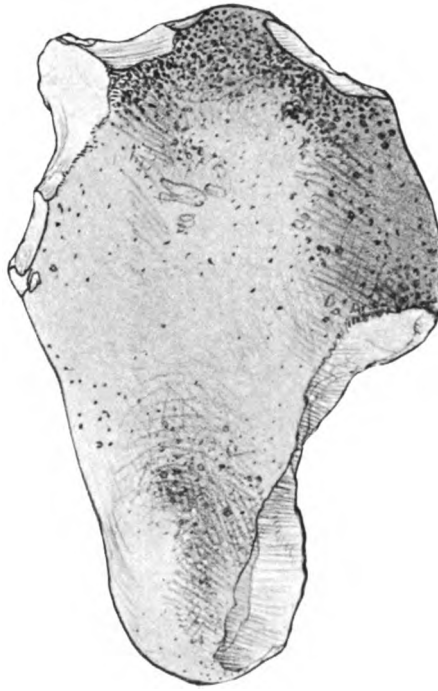


Fig. 2.



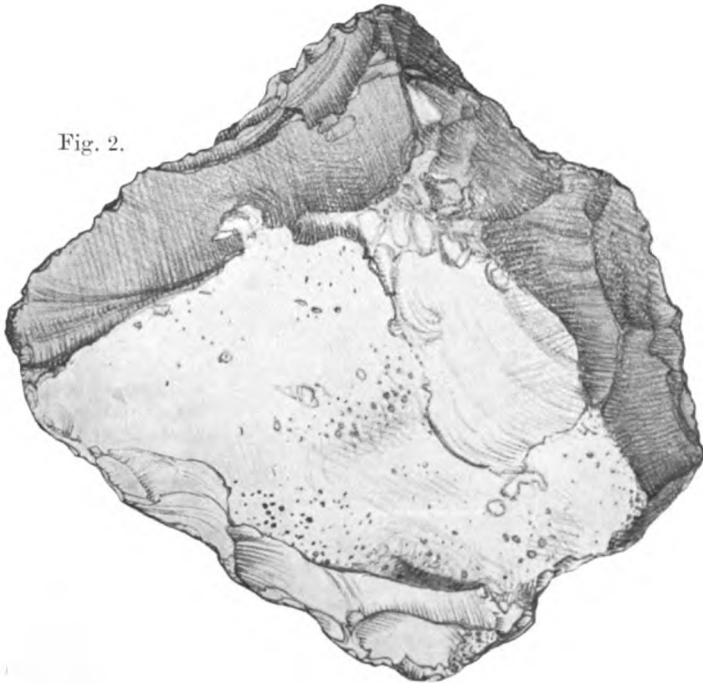
MIDDLE GLACIAL IMPLEMENTS. Group 1.

PLATE III.

Fig. 1.



Fig. 2.



MIDDLE GLACIAL IMPLEMENTS. Group 2.

PLATE IV.

Fig. 1.



Fig. 2.



Fig. 3.



MIDDLE GLACIAL IMPLEMENTS. Group 3.

PLATE V.

Fig. 1.



Fig. 2.



MIDDLE GLACIAL IMPLEMENTS. Group 4. (Reduced $\frac{1}{4}$).

PLATE VI.

Fig. 1.



Fig. 2.



IMPLEMENTS FROM THE CHALKY BOULDER CLAY. (Reduced $\frac{1}{2}$).

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particular implements was dying out at this period. The flaking also is of a different order to that showing on the Middle Glacial and Sub-Crag specimens. In each illustration the left lateral surface of the implement is shown, and the apex of the beak is on the extreme left-hand side of the drawing.

The following is a full description of these rostro-carinate implements :—

PLATE I., *Fig. 1.*—Found by J. Reid Moir in the undisturbed bone-bed below the decalcified crag in Messrs. Bolton and Laughlin's brickfield, Ipswich.

The flaked surfaces are a rich, dark, brown colour, and exhibit a high glaze.

The edges are slightly bruised, and a few small scratches and incipient cones of percussion (the latter are caused by the impact of one stone upon another) are developed upon the upper surface of the butt end of the implement.

The reverse face of the flint is flaked in the same manner as that figured, and the ventral or lower surface has been formed by two blows from the apical end of the implement.

PLATE I., *Fig. 2.*—Found by J. Reid Moir, 15 feet from the surface in Middle Glacial Gravel capped by Chalky Boulder Clay, in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces are a light yellowish-brown colour, and exhibit a few incipient cones of percussion.

The edges are a little abraded, but no scratches appear upon the implement. The reverse face is flaked in the same manner as that figured, and the ventral or lower surface has been produced by a blow removing a flake of a concave shape.

PLATE I., *Fig. 3.*—Found by Baxter (the quarryman employed by Mr. Moir) in the Chalky Boulder Clay exposed in Messrs. Bolton and Laughlin's pit situated on the plateau to the north of Ipswich.

The flaked surfaces are quite black and unpatinated and exhibit neither scratches nor incipient cones of percussion.

The reverse face exhibits one large surface of fracture, and the ventral or lower surface is composed partly of the natural cortex of the flint which, however, has been somewhat cut away to produce the overhang of the beak.

PLATE II.—SPECIMENS OF MIDDLE GLACIAL GRAVEL IMPLEMENTS : GROUP I.

Fig. 1.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces exhibit a dark chestnut-brown colour, and are slightly glazed.

The flakes have been removed at a *high* angle to the edge, and the implement shows some little amount of abrasion.

A few scratches appear upon its under surface which has been formed by a single blow ; no incipient cones of percussion are to be seen upon this specimen.

The flaked surfaces have a peculiar pock-marked appearance, and dendritic markings are developed upon the cortex of the flint.

Attention is drawn to the almost exact similarity, in form and mineral condition, these specimens bear to the "eolithio" borers from the high plateau gravel of Kent.

Fig. 2.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces exhibit a dark chestnut-brown colour, and are slightly glazed.

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The flakes have been removed at a *high* angle to the edge, and the implement shows some amount of rolling and abrasion.

No scratches appear upon its surface, but some few incipient cones are developed; also small dendritic markings are to be seen upon portions of the cortex.

The reverse face is nearly flat and has been formed by two blows.

PLATE III.—SPECIMENS OF MIDDLE GLACIAL GRAVEL IMPLEMENTS: GROUP II.

Fig. 1.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces, which give to the implement a definite "scraper" form, are of a darkish chocolate-brown colour, and exhibit a high glaze.

The flakes have been removed at a *medium* angle to the edge of the flint, which is slightly abraded. The under surface, which has been produced by one blow, is extensively scratched, but only a few incipient cones of percussion appear upon the specimen.

Fig. 2.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces, which give to the implement a definite pointed form, are of a darkish chocolate-brown colour and exhibit a distinct glaze.

The flakes have been removed at a *medium* angle to the edge of the flint, which is very slightly water-rolled.

The reverse face of the specimen is flat and has some flakes removed from the opposite side of the edge to that from which those showing on the upper face were struck.

No incipient cones of percussion are to be seen, and only two minor scratches.

PLATE IV.—SPECIMENS OF MIDDLE GLACIAL GRAVEL IMPLEMENTS OF GROUP III.

Fig. 1.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces, which give to the implement a definite beaked shape, are of a greyish-blue colour, and exhibit a medium glaze.

This specimen is entirely "flaked out," and no cortex is to be seen upon it. No scratches are to be seen upon any of the surfaces, but a few incipient cones of percussion are developed.

The implement is slightly abraded, and the reverse face is flaked in a similar manner to that figured.

The apex of the beak is on the extreme right-hand side of the drawing.

Figs. 2 and 3.—Both found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's sand pit, Ipswich.

The implements, which are both "scrapers" made from struck flakes, are of a greyish-blue colour.

The two reverse faces are both plain bulbar surfaces, and neither of the implements show any definite signs of water-rolling.

Neither scratches nor incipient cones of percussion are developed upon their surfaces.

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PLATE V.—SPECIMENS OF MIDDLE GLACIAL GRAVEL IMPLEMENTS : GROUP IV.

Fig. 1.—Found by Lieut.-Colonel Underwood in the Middle Glacial Gravel at Messrs. Bolton and Laughlin's sand pit, Ipswich.

The flaked surfaces, which give to the implement a somewhat palæolithic appearance, are of a beautiful *café au lait* colour and exhibit a very high lustre.

The reverse face, which has been produced by blows, exhibits a few scratches, but no incipient cones of percussion are to be seen upon the specimen.

The implement shows very little, if any, signs of water-rolling.

Fig. 2.—Found by J. Reid Moir in the Middle Glacial Gravel in Messrs. Bolton and Laughlin's pit, Ipswich.

The large flaked surfaces, which give to the implement a somewhat palæolithic appearance, are of a beautiful *café au lait* colour and exhibit a high glaze.

The reverse face of the implement is flaked in a similar manner to that figured.

No scratches or incipient cones of percussion are to be seen upon this specimen, and it is only very slightly abraded.

PLATE VI.—IMPLEMENTS FROM THE CHALKY BOULDER CLAY.

Fig. 1.—Found by Baxter in the Chalky Boulder Clay, 10 feet from the surface, in Messrs. Bolton and Laughlin's pit on the plateau to the north of Ipswich.

The implement is of black, unpatinated flint, and shows only a slight lustre.

The reverse face is formed by a single blow, and the specimen exhibits neither scratches, incipient bulbs of percussion or signs of water-rolling.

Fig. 2.—Found by J. Reid Moir in the Chalky Boulder Clay in Messrs. Bolton and Laughlin's pit on the plateau to the north of Ipswich.

The implement is of black, unpatinated flint, and shows a slight lustre; iron stains appear on some of the ridges where two facets meet.

The reverse face is formed by the same large flakes as on that figured, and the specimen exhibits only a few incipient cones of percussion and two small scratches.

Some of the ridges are slightly battered.

Attention is drawn to the approximation of these Boulder Clay specimens to the earliest Chelles-palæolithic implements.

In all the drawings the shaded portions simply represent the appearance of the implements as seen by the artist, and do not indicate that the specimens so shaded have been "reworked."

The cortex or crust of the flint is indicated by the dotted areas.

Except where stated all the implements are drawn of the natural size.

All the implements figured are exhibited in the Ipswich Museum, and can be seen and handled by all who wish to do so.

MORE MENDELISM AND MIMICRY

By Professor Punnett, F.R.S.

IN an article recently published in the pages of this journal I ventured to try and defend myself against an attack made upon me by Professor Poulton, and perhaps it was inevitable that the attempt should have involved some criticism of the theory of mimicry of which he is so well known an exponent. "Nur der Streit enthielt die Wahrheit," wrote a distinguished morphologist some years ago, and it was in that spirit that I replied to Professor Poulton's onslaught. For some time past I have felt that there were grave difficulties in accepting the current theory of mimicry, and I hoped that by attempting to formulate some of these difficulties, I might elicit, if not a complete answer, at any rate some evidence that a serious attempt was being made to meet them. The nature of some of these difficulties may perhaps be made clearer if I quote a paragraph from my former article.

"It is an hypothesis which, in its present form as advocated by Professor Poulton and others, confers upon minute variations a selective value which is inconceivable when regard is had to the nature of the selecting agent ; it makes the sweeping assumption that such minute variations are inherited : it is driven to argue for an utterly unknown and mysterious process by which these minute variations can be built up into a widely different and fixed form : it is unable to account for the absence of transitional forms when the germ-plasms of the old form and the new one are mixed : it has no adequate explanation to offer for the frequent absence of mimicry in the male sex : it leaves without any solution those numbers of cases of polymorphism where there is no question of mimicry : and lastly it endows birds

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with powers of selective destruction which are certainly not deducible from the available evidence."

I had hoped that these various difficulties might be taken one by one and some suggestion offered as to the way in which each might be met. It was therefore with a feeling of disappointment that I read Professor Poulton's contribution to the last number of *BEDROCK*. But before I pass on to consider the arguments there adduced, I cannot refrain from protesting against the spirit in which the opening paragraphs are written. I have never wittingly made "assertions which imply that Darwin was a hasty generaliser, that his opinions on fundamental questions were ill-considered and of no importance." That he was inclined to attribute too much to the operation of Natural Selection I certainly believe; but had he possessed the knowledge now available for us, I feel sure that he would have been the first to welcome it and to assign to it its proper value. Charles Darwin's work is not beyond fair criticism any more than that of any other man, and those who know his writings may feel sure that he himself would be the last to wish it otherwise. To seek to stir up prejudice against an opponent in this way is surely unworthy of controversy in which the aim is to arrive at truth. It is not the first time that Professor Poulton has attempted to do so, but I trust it may be the last.

And now we may turn to the way in which Professor Poulton deals with the difficulties which I raised—or more correctly I should say with one of those difficulties, for I cannot find that he has anything of much value to say about the others. In selecting the question of the inheritance of small variations he has judiciously chosen one of great importance in the present connection though unfortunately clouded with much misunderstanding in the matter of terms. Some years ago I stated that de Vries was to be credited with having drawn a distinction between two forms of variation, viz., variations of the nature of mutation which were of value in progressive evolution, and variations which were not heritable, or fluctuations. For this, I, and others, were criticised by Professor Poulton who pointed out* that, though de Vries was often ambiguous, yet on the whole he used the term "fluctuation" for variations

* *Darwin and the "Origin,"* 1909.

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which were transmissible up to a certain point, provided that rigid selection was exercised. But as soon as selection ceased the material began to relapse into the condition in which it was before the selective process started. I freely admit that in attributing to de Vries the credit for drawing the distinction between variations which are transmissible and variations which are not transmissible, I may have erred. But in making that attribution I had neither the intention nor the wish to shield myself behind authority. I believe the distinction to be a real one, and in so far as I am concerned I am perfectly willing to take the burden of it upon my own shoulders. In the light of existing evidence, then, I am of opinion that two kinds of phenomena are classed together under the general term variation, viz., (1) those dependent upon mutations which arose suddenly in the germ-plasm and are thenceforward subject to the ordinary rules of inheritance, and (2) those which are not transmissible, but depend upon the fact that no two individuals, or the gametes which went to form them, can be exposed to precisely the same conditions during their respective lives. I trust that I may be forgiven by Professor Poulton if I continue to use the term fluctuation for this latter kind of variation. No doubt Natural Selection will see to it that my term is eliminated if it is found unfit.

There still remains the question of what de Vries' "fluctuations" really are. From our present standpoint a clear understanding of these is important, for I gather from the general trend of Professor Poulton's article, although I cannot find an explicit statement, that it is upon such variation that his case for the production of mimetic forms through the agency of Natural Selection is largely based. The likeness of mimic to model is kept up by means of the continual operation of this factor, and if its operation ceased I gather that the likeness of the mimic to the model would gradually become less marked. In illustration of such variation Professor Poulton has chosen de Vries' case of the sugar beet. It is well known that during the last half century the percentage of sugar in the juices of this root has been increased from about eight to over fifteen. According to de Vries, the sugar beet exhibits fluctuating variability in the percentage of sugar, and this variability is transmissible. By continually selecting the best roots as parents the percentage has been gradually raised until, for some unknown reason, it reaches a limit.

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At this juncture continued selection is necessary to keep up the percentage of sugar even though it cannot increase it, and unless such selection is exercised the sugar percentage gradually drops to what it was before the process began. Theoretically, as well as practically, such a conception of the nature of variation is of the first importance, and if substantiated by facts which have been fully analysed it would lend force to Professor Poulton's contention that the closeness of resemblance between mimic and model is really due to the operation of Natural Selection, and that it is only by the continued operation of the selection process that the resemblance is maintained. But before we commit ourselves to such a view of the nature of variation, I feel that we ought to have conclusive evidence that no other interpretation of the facts is admissible. In the case of the sugar beet, perhaps the best example hitherto adduced in support of the existence of this type of variation, the evidence is by no means so conclusive as de Vries would have us believe. Professor Wood* has pointed out that the process of improvement is not the result of a uniform method of selection. It consisted at first in picking out the best-looking roots. Later on roots of high specific gravity were chosen as seed parents, and still later the selection was for high specific gravity of juice. Then came the introduction of the polarimeter, by which the breeder was enabled to pick out the roots with the highest possible percentage of sugar without an increase in the total solids contained in the juice. In this way the sugar content was increased from about 11·8 per cent. in 1870—2 up to 15 per cent. in 1886, and since then it has remained much the same. Analysis of the case shows that the improvement has been brought about mainly by alterations in the technique of the selective process. It is not so much a steady, gradual change as a series of steps whose origin may be traced to definite alterations in the technique of selecting. The available evidence does not preclude the possibility that we are dealing with "pure lines" in the sense in which Johannsen used the term in his experiments with beans, and as some of the readers of this article may not be familiar with the work, I may recall briefly the conclusions to which he was led.

If we take a bag of beans and weigh each individual seed, and if we then plot a curve such that the ordinates represent the number of

* *Journ. Agr. Sc.*, 1907.

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individuals and the abscissæ the various groups of bean weights, it will be found that the curve will probably be that of a normal curve of error (cf. Fig. 1). If, year by year, the larger seeds are picked out for the production of the next generation, the mode of the curve will be gradually shifted towards the direction of increased weight, or *vice versa* if the selection exercised is in the direction of smaller seeds. But, as in the case of the sugar beet, there are natural limits in either direction which even the most persevering selection is unable to force the beans to transgress. Owing to the fact that the bean is a self-fertilised plant Johannsen was able to push the analysis of this case further than has hitherto been found practicable in the case of the sugar beet. It was shown by Mendel years ago

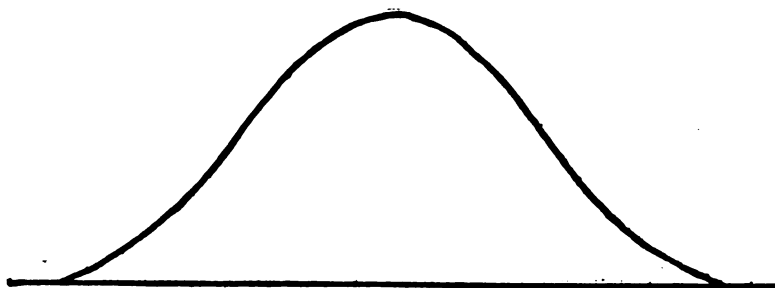


Fig. 1.

that a habitually self-fertilised plant, even if of hybrid origin to start with, must eventually be represented by pure breeding or homozygous strains. By growing on numbers of individuals among his bean population, and by keeping the progeny of each individual separate, Johannsen was able to show that the population really consisted of a number of independent strains each with its own mean and range of variation. The continuous curve by which the general average and the range of variation of the individuals in the bag of beans may be expressed is to be regarded as composed of a number of component curves (Fig. 2, A—J), each with its own mean and range of variation.* With regard to these components or “pure lines,” the experiments of Johannsen have elicited an interesting and important point. Selection within a pure line has no effect in altering the average weight. Variation within the pure line may be

* For the sake of simplicity I have supposed that there are only ten components. Johannsen actually found a larger number.

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considerable, and in some cases the smallest beans are barely half the weight of the largest. Nevertheless, the selection of the smallest over a series of years had no influence on the average weight of the progeny any more than had the selection of the largest. The average weight of the beans belonging to a pure line depends upon the genetic properties common to all the beans of that pure line. Individual members may exhibit great fluctuations in size, but these fluctuations are not transmissible.

Moreover, this conception of the nature of a bean population provides an explanation of the fact that selection applied to it can bring about a change in the average weight. This happens because the larger beans (assuming that selection is directed towards increased

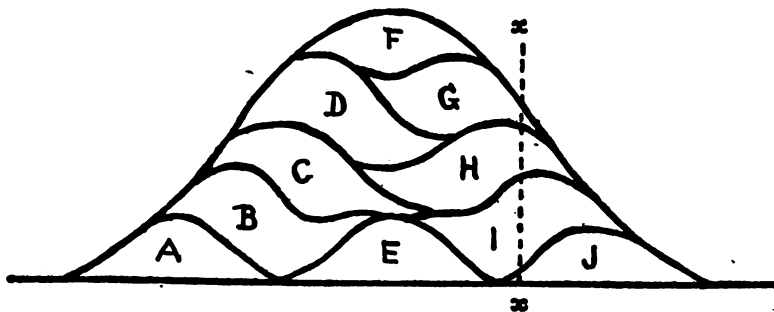


FIG. 2.

size) belong, on the whole, to the pure lines with the higher average weight. If, in our example (Fig. 2), the heavier beans to the right of the dotted line $x \dots x$ are chosen as parents, the average weight of the next generation will be markedly increased, because these heavier beans used as parents belonged to the four pure lines G, H, I, J, of inherently heavier average weight. Some of the pure lines of lower average weight are at once eliminated. And if the process be continued, the lines G, H, and finally I, will tend to become eliminated until we are left with nothing but line J. And there the process would end. By continued selection we should change the average weight of our beans from the average weight of the general population we started with to the average weight of the pure line J. The process would be rapid at first and then increasingly slower, if, as usually happens, an appreciable number of seeds were taken each time. But no amount of selection would take us beyond the average

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weight of the line J. Selection alone does not extend the limits of our original variation curve. It merely shifts the mean from one part of the curve to another. No new type is created by selection. It merely selects a pre-existing variation, and eventually disentangles it from others with which it was mixed up.

It is not unlikely that the phenomena which Johannsen was able to demonstrate for his beans may also be shown in the sugar beet when the case has been carefully analysed. The fact that selection has brought about an increase in the sugar content is not in dispute. Neither would it seem to be disputed that the sugar content cannot be raised beyond a certain point by selection alone. There is, however, a point of difference in the accounts of the two cases, in that it is said that the sugar content of the beet gradually recedes unless the selective process is continuously applied. But it must not be forgotten that the beet is a plant in which cross-fertilisation occurs, and also that in-breeding may play a part for which there is no parallel in the bean. Until, therefore, the variation of the sugar beet has been analysed with the same care as in the bean, I do not consider that we are justified in assuming the existence of the type of variation which de Vries terms "fluctuating." It will be time enough to discuss the nature of de Vriesian fluctuations when we are in possession of carefully analysed evidence which cannot be interpreted on the lines that recent experimental work has led us to believe are sound. Pending the production of such evidence, we may leave out of account any argument of Professor Poulton's which is based upon the assumption that variation of this nature exists.

We are left then with two classes of variation, (1) those which I have termed fluctuating, which are not represented in the germ-plasm, and are therefore of no account for evolutionary change, and (2) those which have been called "mutations," which are represented in the germ-plasm, and are subject to definite schemes of transmission. Variation belonging to either of these two classes may be "large" or "small," but, of course, in using such a qualification we merely record an ocular sense impression and make no assumption as to whether the alteration in the condition of the germ-plasm bears any relation to the "size" of the mutation exhibited. For all we know, a "small" mutation may involve a far more radical germinal change than a "large" one. However, it is

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probable from such evidence as we have at present that larger variations are more likely to be of mutational nature, while fluctuations account for the greater part of the infinite numbers of small variations. To distinguish between the two kinds there is at present but one method open to us, and that is the method of experiment. Only in this way can we decide whether a variation is inherited or not—whether it is of the nature of a mutation or of a fluctuation. Heredity may be regarded as a mode of analysis by which we can gain an insight into the constitution of the living thing, and by means of which alone we can decide what variations are of evolutionary value and what are not.

Unfortunately, direct experiment is a matter of great difficulty in many of the cases which form the subject of the present controversy. Professor Poulton has, however, brought forward an argument for the transmissibility of small variations in the relative constancy with which local races of the same species exhibit slight differences in pattern. He points out, for example (p. 300), that in *Danais chrysippus* (a species figured in my previous article in BEDROCK) a white spot occurs in a certain position on the forewing in specimens from India and Ceylon. Eastwards, in the region of Hong Kong, the spot tends to become larger and confluent with the white bar on the forewing; westwards, in Africa, it tends to disappear altogether. And, to quote Professor Poulton,

“Other minute geographical changes in the same butterfly might be described, and, as I have said, any number in other species. The only question that remains is their transmissibility, but I imagine that Professor Punnett will hardly doubt that each local pattern of a butterfly, occupying corresponding stations in the different parts of its total habitat, is a hereditary pattern.”

But I certainly doubt whether these markings are necessarily hereditary in the sense that Professor Poulton means. It seems to me not at all unlikely that the differences are what are often vaguely termed climatic—that the larger white patch on the Hong Kong forms, or its absence in the African ones, is due to the butterflies living under a somewhat different set of environmental conditions. These differences of marking may quite well be of the nature of fluctuations, induced by climatic differences in the regions where these geographical races live. They may appear as racial characteristics because the successive generations grow up under approximately

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the same environmental conditions. The matter could be tested by taking examples from one locality, say Africa, and breeding them in another, *e.g.*, Ceylon. If the progeny retained their African pattern under the Ceylon conditions it would afford the strongest evidence in favour of a genetic difference between the two forms. If, on the other hand, the progeny of the transplanted Africans took on the pattern of the Cingalese, there would be a strong presumption in favour of regarding these local differences as merely fluctuations. The differences may be hereditary or they may not be, but until experimental evidence is available I do not think we are justified in looking upon them as necessarily hereditary in any given instance.

In this connection the experiments of Merrifield and others on seasonal dimorphism are certainly suggestive. The little butterfly *Vanessa levana* occurs in two distinct forms which, in appearance, are widely different from one another. One form, *levana* proper, emerges in the spring, and is not very different in general pattern scheme from the familiar small tortoiseshell. During the late spring it pairs and gives rise to another brood which emerges later on in the summer. The individuals of this brood are of the *prorsa* form, which is very different in appearance from the normal *levana*. Instead of the bright chestnut brown mottled with black, the fly is mainly black, with a rather broken whitish band running down both fore and hind wings. *Prorsa*, in turn, gives rise to a brood which goes over the winter, and eventually emerges as *levana* in the following spring. Under normal conditions the two forms alternate with one another, *levana* emerging in the cooler spring and *prorsa* in the warmer summer of the same year. It has been known for some time that the species is very sensitive to temperature conditions during its later larval and earlier pupal life, and the work of various experimenters, especially that of Merrifield,* has shown that the emergence of the *levana* or the *prorsa* form is determined by the conditions of larval or pupal life. The eggs laid by *levana* give rise normally to *prorsa* because the sensitive stages are passed through under warm conditions. But by artificially cooling at the appropriate stages *levana* can be made to produce *levana*. So also, by forcing at a warmer temperature, *prorsa* can be

* Report of 1st Internat. Entom. Congr., Brussels, 1910.

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made to produce *prorsa* in place of the *levana* which it gives under normal conditions. Moreover, by suitable adjustment, an intermediate form, *porima*, can be produced. Briefly, the resulting pattern of the butterfly is a function of the conditions, and primarily of the temperature, under which certain sensitive stages of its earlier life are passed. Were the species single-brooded in Europe, probably it would be represented by the *levana* form alone, with perhaps an occasional approach to the *porima* form when the conditions were favourable. Were it able to exist in some tropical country, the *prorsa* form alone would probably be met with, and the two geographical races might even be classified as distinct species. Each would breed true to its own particular form *under a given set of conditions*. The difference between them, striking as it is, does not depend upon a difference in genetic constitution. It is not a hereditary difference, but merely an outward record of the fact that they have grown up under different conditions.

I have dwelt upon the case of *Vanessa levana-prorsa* because the fact that a species, without alteration in genetic constitution, may breed true to one pattern under a given set of conditions and to another pattern under another set of conditions, has a definite bearing upon Professor Poulton's argument that the constancy of local varieties implies that their differences are necessarily hereditary. As has been already pointed out above, Professor Poulton uses such cases as that of *Danais chrysippus* in support of his contention that if a form breeds true to a given character, that character must necessarily have some specific* representation in the genetic constitution of the form in which it occurs. Such cases as that of *Vanessa levana-prorsa*, and there are many of them, indicate that this is by no means necessary. Until we have experimental evidence to the contrary, I see no cogent reason for regarding the differences in the white spots of the geographical races of *Danais chrysippus* as other than non-transmissible fluctuations directly dependent upon the action of the environmental conditions. It may be that they have a genetic foundation. But I feel that the burden of proof lies with those who consider them to have such a foundation, because

* May I venture to hope that Professor Poulton has familiarised himself with the uses of this word, since he recently criticised my application of it. (Cf. BEDROCK, Vol. II., p. 303.)

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I am of opinion that small variations are more commonly of the nature of fluctuations than of heritable mutations. Certainly I do not think we are yet justified in drawing from such cases the conclusions which Professor Poulton seems to consider should be drawn by everyone as a matter of course.

So far I have, out of deference to Professor Poulton, followed his lead and confined myself to the general topic of variations, transmissible and non-transmissible. In our present state of ignorance any discussion of the problems involved is bound to be largely speculative. Critical cases are so rarely to hand when wanted, and one can do little more than suggest lines along which explanation may perhaps be profitably sought. It is, then, with some feeling of relief that I find myself at liberty to pass from this evasive subject to that portion of Professor Poulton's article which bears more directly upon the subject of our controversy, which, after all, is mimicry. In the July number of *BEDROCK* I pointed out that the hypothesis of mimetic resemblances being brought about by the gradual accumulation of small variations must often land us into absurd deductions as to the ancestral pattern of a model which has several mimics. Evidently the argument went home, for it has elicited from Professor Poulton the clearest and least equivocal statement that I can remember from him with reference to the origin of mimetic resemblances.

"I have always recognised," he writes, "that the first variation must be something appreciable, something which, at any rate, at a distance and on the wing would recall the pattern of the model."

As I read that sentence there stole over me a feeling of perplexity. Was Saul also among the prophets? Was this the voice that but four years ago declared that—

"As the mutationist comes to study the details of adaptation . . . the belief in an evolution founded on large mutations will vanish, and we shall then come back to mutations identical in every respect with the small variations which were for Darwin the steps of evolution." *

It was not until I had come to the end of Professor Poulton's article, until I had read his claim to "offer what a Darwinian really

* *Darwin and the "Origin,"* 1909, p. 279.

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believes for what he is assumed to believe," * that I felt I was beginning to get an inkling of the true state of affairs. But even now I cannot understand why he has found it so difficult during all these years to emphasise what he really believes. However, it would be ungracious of me to cavil at an admission on the part of Professor Poulton which goes so far to bring his views into agreement with my own. We are both agreed, then, that in origin these striking resemblances called mimicry are founded upon a mutational basis, and that until the mimic can be mistaken for the model, Natural Selection plays no part whatever in the process. And there can be no doubt that in many cases, at any rate, we are forced to suppose that the original mutation must be such as to bring about a very considerable change in the appearance of the insect. Take, for instance, the case of two butterflies figured in my last article—*Danais plexippus* and its Satyrine mimic, *Elymnias undularis*. In the latter species the sexes are very different in appearance—the male is a dark purple-brown fly with a little brown on the hind wings. The general colour of the female is bright yellow-brown, with darker margins to the wings, on which are conspicuous patches and spots of white. In general appearance it strongly recalls *Danais plexippus*, and has long been regarded as a classical instance of mimicry. Adopting Professor Poulton's view that the female was originally like the male,† the question we have now to try and decide is how great a mutational step is required to produce a form from the purple-brown male pattern, which can be confounded with *Danais plexippus*. I do not see how Professor Poulton, or anybody else, can doubt that, as far as visible appearance goes, the step must be a very considerable one. Nor is it possible in this case to hedge

* I take it from the following passage that Professor Weismann, another staunch Darwinian, does really believe what he is assumed to believe: "The great likeness of these whites to the Heliconiidae, Bates further argued, would depend on a process of selection, based on the fact that, in each generation, those individuals would on the average survive for reproduction which were a little more like the model than the rest, and thus the resemblance, doubtless slight to begin with, would gradually reach its present degree of perfection." *The Evolution Theory*, London, 1904, Vol. I., p. 92.

† Let me state here that this is Professor Poulton's view, and that I am not prepared to subscribe to it without reserve. It appears to me by no means impossible that in some at any rate of these cases of sexual demorphism the male form may be the more recent in phylogenetic sequence.

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by invoking " the alternating light and shade of a tropical forest " as a factor for accentuating a poor resemblance. Both *Danaïs plexippus* and *Elymnias undularis* fly freely in the open, and may be taken plentifully in the streets and gardens of Colombo and other large towns of Ceylon. We are forced, therefore, to suppose that the resemblance between the female of *Elymnias undularis* and the two sexes of *Danaïs plexippus* is dependent in the first place upon a mutation of very considerable size implying so great a change as the sudden origin of a form which can be confounded with *Danaïs plexippus* from the widely different form of the male of *Elymnias undularis*. Moreover, Professor Poulton grants that this change, from its very nature, must be quite independent of any operation of Natural Selection.

Now I submit that, if the initial mutation upon which a mimetic resemblance is founded can be of such magnitude, it can be sufficiently large to produce the resemblance as we know it at present. If a form which is sufficiently close to *plexippus* to be confused with it can arise suddenly by a single step, what grounds have we for denying that this single step has resulted in the female of *Elymnias undularis* as we know it to-day ? So far as I can see there is nothing whatever against it, unless one is afflicted with the desire to drag in Natural Selection at all costs. I have taken the case of these two species partly because it is one of the clearest I can think of, and partly because I happen to have been familiar with the appearance of the living insects. But it is evident that the argument is of general application.

The last portion of Professor Poulton's article is devoted to the case of *Papilio polytes*, with its three forms of female, which were figured in the July number of BEDROCK. In that number I argued that the fact that from appropriate matings any form of female could produce any other without intermediate forms was strong evidence for supposing that the differences were of mutational nature, and had arisen suddenly at some time or another. I further pointed out that this position has been recently strengthened by the important experiments of Mr. Fryer,* who had been able to offer a simple Mendelian interpretation for this interesting case.

* *Phil. Trans.*, 1913.

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Whatever the genetic differences between any two of these three forms of female may be, their hereditary behaviour is such that they may be expressed in terms of a definite genetic factor following the regular scheme of heredity with which we are now so familiar.

That being so, it is natural to suppose that the origin of the difference was due to a sudden alteration in the germ-plasms involving the loss or gain of the factor * upon which the difference depends. My argument was that since the pattern differences *breed* in each case as if they were due to a single entity, we must suppose that they *arose* in each case through the gain or loss of a single entity—in other words, that they were produced in their present form. It is with a peculiar and original criticism that Professor Poulton meets this argument of mine :

“Men with potential aptitude—more or less—for reasoning are born every day. Does Professor Punnett therefore believe that man as he is now arose suddenly from a common ancestor with the anthropoids ? If not, the fact that the different females of *polytes* are produced now is hardly an argument that they were originally produced in their present form.” †

May I venture to point out to Professor Poulton that these two cases are not necessarily parallel. It has first to be shown that in matings between men and anthropoids the grade of reasoning power characteristic of the two species segregates cleanly without the production of intermediates. If this were shown to occur as the result of a union between a white man and a female gorilla, I should be willing to consider the case in this connection. Meanwhile the burden of proof evidently rests with Professor Poulton.

As I do not feel satisfied from Professor Poulton's answer that he has grasped my argument, and as the point is an important one, I may illustrate it with a case from another species. He may, perhaps, view the rabbit with a less prejudiced eye than he looks upon the butterfly. For several years past my friend Mr. P. G. Bailey and I have been investigating the inheritance of certain pattern characteristics in the domestic rabbit. In the so-called Dutch rabbit the

* It is, of course, conceivable that the “factor” may be a complex of several factors showing complete coupling. But on the existing evidence we have no grounds for assuming the existence of more than a single factor in each case.

† BEDROCK, October, 1913, p. 305.

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coat is partly pigmented and partly white. Roughly the anterior part of the animal is white except the ears and a patch round each eye, while the hinder part is pigmented except the hind feet, which

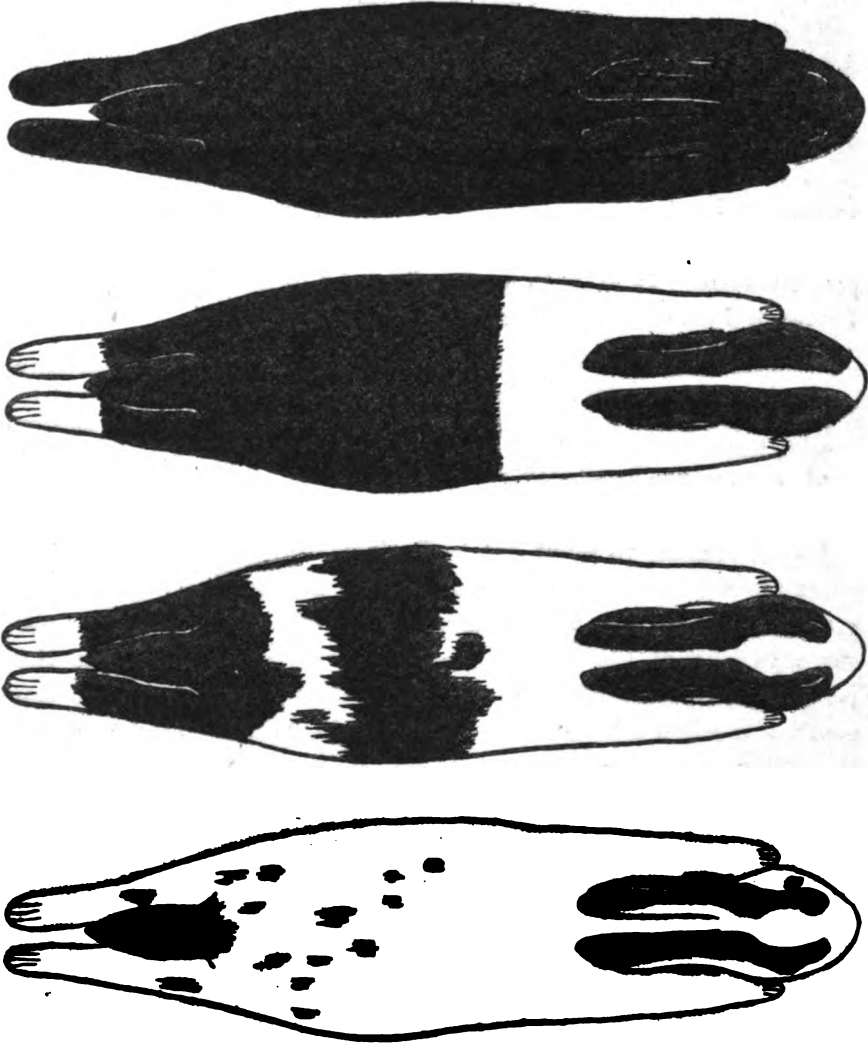


FIG. 3.

are white (cf. Fig. 3, B). In other animals the pigmented areas are rather more restricted and the hinder part tends to be invaded with white (Fig. 3, C). In other animals, again, the reduction of the pigmented areas is much more marked throughout and the animal

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is predominantly white (Fig. 3, D). Including the fully or almost fully pigmented animal we have, therefore, four classes, and on the whole the animals can be readily grouped into these four classes, although all of them show a good deal of variation. Breeding tests have shown that D breeds pure, that C can throw D, but not A or B, that B can throw C and D, but not A, while A can throw any of the four classes. We may regard this, then, as a series of stages passing from the completely pigmented coat to a stage in which the coat is predominantly white. It is not unlikely that the three forms B, C, and D have arisen in this order from the self-coloured form, though of this we have no direct evidence. From D breeds true, nevertheless when it is crossed with the self-coloured form there come in F_2 , in addition to A and D, both B and C. Where evolution of one form from another has occurred as a series of steps, the result of mating that form with the form from which it sprang in the first place is to produce again the series of intermediates. I have chosen the case of the rabbit in illustration of my thesis because it lends itself easily to illustration, but plenty of similar cases could be quoted both from plants and animals.

Now if, as Professor Poulton wishes to have us believe, the transition between the male form of *Papilio polytes* and the "aristolochiæ" form (BEDROCK, July, 1913, Plate II.) has been brought about by a series of independent steps, why do we get no evidence of this when the two forms breed together? The cross is happening every day. Great numbers of the butterflies have been examined; yet an intermediate which cannot be classified either as the male form or the "aristolochiæ" form has never, to my knowledge, been found. Neither does Professor Poulton's contention, that because the difference between two patterns is somewhat complex it cannot have arisen as a single mutation, appear to me to constitute a serious objection. The black and tan rabbit differs from the self-black in a number of points. The visible difference between them is complex. Yet their genetic behaviour when crossed can be expressed in forms of a single factor, and I have little doubt but that the experienced fancier would regard the one form as having sported suddenly from the other. Breeding tests have demonstrated that the difference between the germ-plasm of the "aristolochiæ" and the male forms of *Papilio polytes* must be expressed in terms of a single factor, and

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when we come to ask ourselves how that difference has arisen I do not see how we can fail to draw the conclusion that it was by the gain or loss of a single factor. I submit that the genetic behaviour of the forms when crossed is at present the only safe ground upon which to draw inferences as to the manner in which they have arisen.

Professor Poulton has elaborated at length his conception of the stages, four in number, by which the passage from the male to the aristolochiæ form has been brought about. But of course it must be recognised that this is merely a personal opinion entirely unsupported by a shred of evidence either from intermediate forms or from the genetic behaviour of the forms when bred together. And here I may dwell for a moment upon another point. Professor Poulton assumes throughout that the male form is the ancestral one, and that the mimetic forms have arisen from it. This interpretation of the course of evolution may be right, but I do not think that it is necessarily inevitable. Mr. Fryer's work suggests strongly that the "aristolochiæ" female has a factor more than the male form of female, and that the factor form again has a factor more than the aristolochiæ form.

The genetic studies of the past few years lead us to suppose that the new form more commonly arises by the omission of a factor than by the gain of one. If this were so in the case of *Papilio polytes*, we ought to look upon the "hector" form as the oldest phylogenetically, and the male form as the youngest, thus reversing the generally accepted view. I do not wish to lay stress upon it, but merely to draw attention to the possibility, and to point out that there is no conclusive evidence against it.

Before leaving the subject of this butterfly I must clear up a point on which Professor Poulton seems not to have understood me :

"Granting the sudden origin of the two mimetic forms," he writes, "Professor Punnett admits, on p. 155, that they would be preserved and rendered predominant by Natural Selection, but it is difficult to reconcile this part of his paper with pages 156 to 158, in which he reaches the conclusion that the proportion of the mimetic females in Ceylon expresses a Mendelian equilibrium undisturbed by selection."

I gather that Professor Poulton regards me as having stated on one page that Natural Selection must be operating on *Papilio polytes* in Ceylon, and on another page as denying this statement. If this were really true it would indeed be a grave inconsistency.

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But if Professor Poulton had only paid a little more attention to what I wrote I hardly think he would have brought this charge against me. The sentence referred to on p. 155 runs as follows:—

“Were a new form arising in this way (*i.e.*, by mutation) to bear a close resemblance to some unpalatable species then *I see no difficulty in supposing that it would be favoured by Natural Selection. . . .*” (my italics).

This is rather a different thing to the admission that it must of necessity be preserved and rendered predominant by Natural Selection, with which I am credited by Professor Poulton. My original statement did not commit me to the view that Natural Selection must necessarily come into operation at all. It might or it might not, and I went straight on to consider the possibility of its doing so in the case of *polytes* in Ceylon, and from the statistical evidence available I came to the conclusion that it did not. This conclusion was naturally distasteful to Professor Poulton, but he has brought forward nothing to invalidate it. To talk about the evidence from geographical distribution is not to the point. My argument and the conclusion were based entirely upon the condition of the *polytes* population of Ceylon of which we know something, and I decline to be led off into barren discussions on *polytes* in other parts of Asia where we know little or nothing. If Professor Poulton wishes to impugn my conclusion he must either show that the Ceylon evidence can be interpreted otherwise, or bring forward evidence from some other locality which is at least as ample both on the statistical and the historical sides as that which I have brought together from Ceylon.

May I conclude by expressing the hope that some supporter of the mimicry hypothesis will make a serious attempt to answer the difficulties which I enumerated in my previous article, and again at the beginning of the present one. Professor Poulton's last contribution has left my position unchanged, except in so far as he has strengthened it by the admission that the first step towards a mimetic resemblance must often be a mutation of considerable size. He confined himself almost entirely to the question of the inheritance of small variations, and in the present article I have tried to clear up certain points in connection with this subject where misunderstandings had crept in. But of the most crucial point, the absence

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of transitional forms when the mimetic and the non-mimetic varieties breed together, he has not ventured to offer any explanation. Moreover, we are still without any adequate explanation of the frequent absence of mimicry in the male sex : we are still without any solution of those numbers of cases of polymorphism where there is no question of mimicry : and we are still without incontrovertible evidence for endowing birds with the precise powers of selective destruction which the hypothesis demands. Professor Poulton has not attempted to meet any of these criticisms. Are we then to infer that he cannot ?

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By G. Archdall Reid

I do not know when the controversy as to whether acquired characters are transmissible began. Perhaps, when the too-strenuous troglodyte corrected his wife she may have pleaded the consequences to expected offspring, and he may have expressed a contempt for popular superstition. In any case, the belief that offspring tend to inherit the acquirements of parents was universal, or nearly universal, for uncounted centuries. Lamarck attempted to throw it into scientific shape. Then followed the theory of Natural Selection, which accounted for evolution in another way, and so, in time, inevitably engendered doubt. Thereupon flamed out the greatest of biological controversies. I was early in the ranks of the neo-Darwinians. To-day I am still convinced that the statement that acquirements are transmissible is nonsense; but I am equally convinced that the converse is also nonsense. At first these statements seem to have meaning, but they have none really—no more meaning than a statement that gravitation is blue or not blue, or that a distance has or has not weight.

Before I proceed certain facts must be brought into prominence. They are familiar enough, and are never directly impugned. But by implication they are denied in all manner of biological speculations, even the most modern. It is, I think, demonstrable that the Lamarckian and several controversies persist because these facts are hidden behind that most opaque of veils, familiarity.

The multicellular individual is derived from an ovum (fertilised or unfertilised), a single cell. This is derived, through the cells of the germ-tract, from the ovum whence the parent sprang.* As far

* To avoid circumlocution I speak of only one parent. The argument is not affected thereby. In cases of bi-parental reproduction the reader may think, if he pleases, of both parents.

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as is known the cells of the germ-tract do not conjugate or in any other way receive living elements from the somatic cells, which, therefore—as far as is known—contribute nothing more than shelter and nutriment to the ovum (or sperm) and its cell-ancestors. We have, therefore, every reason to believe that the various parts of the child are not derived from the similar parts of the parent. The whole of him takes origin in the ovum, which, in turn, took origin in its cell-parent of the germ-tract, and so on.

The ovum develops into the individual under the guidance of two influences, nature and nurture. Since in the germ there are neither limbs, nor head, nor anything of the sort, it is plain that the individual inherits from his parent nothing but certain potentialities—potentialities to develop a head, a heart, a scar, a knowledge of Latin, and so forth. The sum of these potentialities is his nature. The natures—the potentialities for development—of individuals differ. Thus some men are naturally taller or darker than others. As a rule, the nearer the “blood-relationship,” the closer is the likeness in nature. Thus a man usually resembles his brothers more closely than he does other people; an Englishman is more like other Englishmen than he is like a negro; and a human being resembles other human beings more closely than a dog.

Nurture is that which awakens nature and converts potentiality into actuality; it is that which causes characters which are only potentialities for development in the germ-cell to become actual characters in the individual. It is the sum of all those influences or stimuli which, acting on the germ-cell, cause the individual to grow from it, and which subsequently cause him to grow from stage to stage till he reaches maturity. The part played by some of these influences is very obvious. Thus a scar results from injury, and a thickened and hardened skin from rough usage. Soil, warmth, moisture, and light are easily recognisable stimuli to development among plants. But some influences are less obvious. Mere nutriment *seems* to be the immediate antecedent to growth in many cases. Thus a child's hair, teeth, and external ears appear to develop simply because it is fed. If they do so develop, then nutriment supplies not only the material—the clay and the straw—for all the child's growth, but also the stimulus for some of it. But teeth and hair do not continue to increase in later life,

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though nutriment is still present. It is likely, therefore, or, at any rate possible, that nutriment is never by itself a stimulus. Probably there is always something else, for instance, an "internal secretion." It is a principal part of the work of the physiologist to discover the nature and action of stimuli. This point, however—as to whether nutriment is, or is not, ever a direct stimulus—is immaterial. That on which I wish to insist is the known fact that there are invariably two factors concerned in *all* development—nature and nurture. They are the two blades of the scissors; without one, the other is naught. Without nature, without the potentiality, without the capacity to develop, the individual could, of course, develop nothing. Without stimulus, all development from an antecedent state would be uncaused—a thing inconceivable. As far as is known—but this point is also immaterial to the argument—the stimulus for the growth of every structure comes from the environment external to that structure.

Compare now the development of an animal (*e.g.*, man) high in the scale of life with that of one (*e.g.*, butterfly) comparatively low in it. Up to birth the human being, dwelling quiescent in the uterus, develops mainly under the influence of stimuli other than use. After birth some of his structures continue so to grow. Thus the development of his teeth, hair, external ears and organs of generation is apparently quite unaffected by use. But many of his structures, for example his voluntary muscles, limbs, heart, kidneys, lungs, brain, apparently develop little if at all after birth, except under the influence of use. We may judge the importance of use from the effect of infantile paralysis on the subsequent growth of a limb, from the atrophy of one kidney under disuse and the increase in size and efficiency of the other under increased use, from the huge growth of a heart that has to work under difficulties (as when the valves are injured), and so on. Most of the structures of the body adjust themselves to their associated structures by developing in response to the strain put on them by these others. In practice we recognise the enormous importance of use to the human being by the games we encourage our children to play. We know quite well that without exercise their bodies cannot attain normal development.

The caterpillar develops in the egg as does the infant before birth

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in response to influences other than use. After emerging from the egg he uses his structures ; but there is no evidence that this use helps in their development. Caterpillars have not the sporting instinct, and an individual which is obliged to move about actively in search for food does not develop, apparently, better than one which leads a more lethargic existence. At any rate, use plays no part in the rapid development that occurs during the chrysalis stage. The perfect insect, like the caterpillar, uses its structures actively, but now again there is no evidence that development is affected thereby. In fact, growth has ceased.

Before birth the mind of the infant is probably almost, if not quite, a blank. Doubtless, especially in the later stages of foetal life, it is capable of feeling ; but in its stable environment there can be little or nothing to feel. After birth, various instincts develop in long succession, for example the instincts to cry when in pain or discomfort and to suck when hungry, and the instincts of hunger, thirst, curiosity, imitativeness, sexual and parental love, and the sporting instinct. An instinct may be defined as a mental impulse or inclination, developed, in the absence of individual experience, to perform a certain action, the instinctive action, on receipt of a certain stimulus. It is a feeling or emotion, distinguished from such an emotion as patriotism or religious fervour by the fact that it is merely awakened by, not created by experience. A man does not learn to feel hungry or to cry or to suck ; he does learn to love his country or religion. Experience, the mental equivalent of use, plays an immense part in the growth of the human mind. The individual learns to walk and talk, and use his limbs dexterously. He gathers a vast fund of knowledge and becomes skilful in thinking about it. Thus he acquires not only the words of a language, not only a vast deal about his universe, but also the power to perform difficult physical (mental, really, for the mind co-ordinates the muscles) and mental feats such as writing, cycling, carpentering, solving mathematical and business problems, and the like. Among his most important instincts are curiosity, imitativeness, and the desire to sport. The last develops both mind and body ; curiosity impels the individual to gather experiences ; imitativeness impels him to learn to walk and speak, and in many other ways to develop likeness to his fellows.

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Some human beings are by nature (i.e., potentiality) mentally incapable of profiting from experience. These, the "perfect idiots," cannot acquire knowledge, or ability to walk and speak, or any kind of mental skill. Lesser degrees of incapacity to profit from experience are termed "imbecility" and "feeble-mindedness." * A genius is a person supremely capable of profiting from experience in one or more departments of mental activity. The great poet, the great artist, musician, general, engineer, mathematician, all learn their trades. All thought, imagination, intelligence, reason, all that is in the intellect, all that is learnt or remembered, almost all our ability to do things, all our power to think about things is a product of this capacity to grow mentally in response to experience. Man has been defined as the thinking animal. There can be no thought, nothing but mere feeling, unless the mind stores something which can be thought about. When the past is a blank, the future is also necessarily a blank, and the present can only be felt. Man has also been defined as the educable animal. An educable being is one who learns. Owing to his capacity to learn, man has invented aids to his natural powers which raise the civilised individual almost as high above the savage as the latter is above the brute. Writing enables him to communicate thoughts and experiences to audiences scattered, or far distant, or to people who will not be born till after he is dead. Books are artificial memories of perfect accuracy and unlimited scope. Mathematics enables him to perform immensely difficult feats of thinking. Tools (e.g., axe), instruments (e.g., telescope), and machinery (e.g., hydraulic press) are similar extensions of his physical powers. Man is a rational being, guiding his future by the light of his past, solely because he learns. He is supremely adaptable because he can develop in this way or that

* The idiot has been defined in all sorts of ways, for example, "Persons so deeply defective in mind from birth or from an early age that they are unable to guard themselves from common physical dangers, such as, in the case of young children, would prevent their parents leaving them alone." But, if the reader considers, he will perceive that the essential feature of the defect is incapacity to grow mentally in response to experience—to learn. Idiocy, a congenital defect, is quite distinct from lunacy, which occurs in later life. The lunatic can learn, but he learns wrongly from experience. Thus common sounds may have an awful significance to him,

As the Lord were walking near
Whispering terrible things and dear.

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according to the experiences he receives—according to the environment in which he grows. He may be a cook or a king. The importance of his capacity to grow mentally in response to experience may be judged by contrasting the normal adult with the idiot and the infant. The idiot cannot learn ; the infant has not learned ; the normal man both can and has learned. In practice we recognise the utility of experience by the home training and the schooling we give our children.

Before the caterpillar emerges from the egg his mind, like that of the foetus in the uterus, is, probably, a blank. But immediately afterwards he is capable of fending for himself. Possessing a full equipment of instincts, he is able, without previous experience, to perform life-saving actions when placed under certain conditions, as when hungry or when in danger. But apparently he learns nothing ; for an old caterpillar seems no better capable of fending for himself than a young one. He has little or no memory. His past, and as a consequence his future also, is a blank. His present is felt, but not thought about. He can feel, but cannot reflect ; he is not intelligent. As a chrysalis he cannot learn anything. As a butterfly he has again from the renewal of conscious life a full equipment of instincts, which do not get supplemented by the products of experience.

Man and the butterfly both develop in accordance with their natures and in response to nurture. But their natures, and, therefore, necessarily, their nurtures are very unlike. The most marked difference lies in the fact that the human being grows to maturity, physically and mentally, mainly under the influence of use and experience, whereas the insect develops mainly, if not wholly, under other stimuli. Many lower animals (*e.g.*, most insects) seem as little capable of responding by physical and mental growth to use and experience as the butterfly. Higher animals, in proportion as they are highly placed in the scale of life, are more capable. Apparently fish can learn a little. Young birds are instinctively able to build nests ; but experienced birds build better. A cat is able to learn a good deal, and can even acquire an affection for familiar people. A dog is more teachable ; he learns so much, and as a consequence is so intelligent, that we make a companion of him. We judge the intelligence of all animals by their power of

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profiting from experience. Our domestic animals must be intelligent in some degree, otherwise they would be uncontrollable—governed purely by instinct, always wild, never tame. Only those animals are intelligent which begin conscious life more or less incapable of fending for themselves, and only such animals are protected and trained by their parents. Manifestly the function of parental care is to supply time and opportunity to develop under the influence of use and experience. It is not needed, and would be useless, in the case of animals that have an adequate equipment of instinct and little or no power of learning. Without some power of learning parents and offspring could not know each other, and family life would be impossible. The greater the helplessness at birth, the greater the lack of instincts and of structures and faculties sufficiently developed to serve the instincts, the greater is the subsequent development under the stimuli of use and experience, and the more prolonged and assiduous must be the parental care. Compare man's helplessness at birth, the magnitude and complexity of his subsequent physical and mental growth, and the prolonged care his guardians bestow on him, with the initial helpfulness and subsequent inferiority of a young house-fly or even of such an animal as a young pig, which can run about immediately after birth and receives for a much shorter time a scantier degree of care.

The higher animals possess some instincts which are not found in lower types, for instance, curiosity, imitativeness, and the parental and sporting instincts. All these additional incitements to action are associated with the power of growing physically and mentally under the influence of use and experience. Thus the sporting instinct is most active in young animals whose structures and faculties have yet to grow and who are not, at the moment, tired (*i.e.*, who have not been using their structures). It is most developed and persists longest in types that grow most under use and experience. In every case it is precisely adapted to develop structures and faculties in the right way. The young kid climbs, the kitten hides and pounces, the puppy chases and fights, the little girl dandles her doll, the little boy delights in contests that foreshadow the grimmer contests of adult life.

As far as I am able to judge, the power to develop in response to use and experience is a late and a high product of evolution.

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Many biologists seem to believe, however, that it is present in some degree even in the lowest animals. No evidence exists that the physical parts of low animals grow through use, but we are told instances in which they are said to have displayed "memory." Unfortunately, this word is used very loosely. Properly speaking it indicates "The mental faculty or power which causes the impressions of bygone events, at ordinary times latent in the mind, to affect it anew, or to be reproduced by an effort for the purpose." A thing is remembered when it is recalled to mind, when it is recollected. This process is quite distinct from "bearing in mind," in which, though something persists in consciousness, nothing is recalled to it. There is all the difference between recollecting and bearing in mind that there is between the reproduction of sounds by a gramophone and the lingering vibrations that come from a struck harp-string. Most of the cases of so-called memory in low animals which have been cited appear to be no more than instances of bearing in mind. Thus, when a house-fly that has settled on one's hand escapes a blow, its swift and erratic flight indicates that its mind tingles with alarm for a space. But presently the flight slows down, and soon, if the fly pass near the hand, it may, attracted by sight or smell, settle as unconcernedly as ever on the danger spot. It is hard to believe that such an animal really learns and recollects.

However, in the present connection, the question whether low animals have, or have not, some power of growing in response to experience is of no importance. It matters not at what stage of life the power had its beginnings. What is important and beyond all question is that it has undergone vast evolution in the higher animals. This evolution, with its homologue, the evolution of the power to grow physically in response to use, is the common, the distinguishing, the essential feature of the differentiation of the higher animals from the lower. It bestows on them their title.

In the higher animals both the potentialities and also the stimuli which co-operate in the development of such characters (*e.g.*, teeth, hair, organs of generation) as do *not* grow in response to use, experience and injury, are present in every "normal" individual, and, therefore, these characters invariably develop. Thus not only has the normal man the capacity to grow a beard, but the conditions are such that if he lives long enough the necessary stimulus

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also is sure to arise. This is true also of all the "normal" characters (*e.g.*, those not due to injury) of low animals. It follows when we find that two individuals differ, for instance, in colour of hair, that we may safely assume that the unlikeness is due to a difference in nature, not to a difference in nurture. So also, when two people, who have used their structures to about the same extent, develop differently, we may assume a difference in nature. Thus the unlikeness between a tall and a short man is probably due to nature, not to nurture. On the other hand, some characters that arise in response to use, and nearly all that arise in response to injury, are not developed by every "normal" individual; for the necessary stimulus is not always present. Thus every man has not the exaggerated muscles of the blacksmith, nor the roughened palms of the navvy, nor the scars of the prize-fighter, nor the knowledge of the scholar. When, therefore, individuals differ markedly in such characters, we may, with confidence, assume that the unlikeness is due to nurture. In practice it is often difficult to distinguish. In every doubtful case we must "judge by the context." It follows further that the breeder and trainer (whether of man or lower beings) has a much greater power of influencing the development in the individual of characters that grow in response to injury, use, and experience than he has of influencing other kinds of characters. Thus, while we can easily endow a child with scars or rough hands, or a knowledge of letters, we cannot so easily influence the size, texture, and colour of his hair and teeth.

The power of responding by growth to the stimulus of injury occurs in much greater degree among plants and low animals than among higher animals. The latter can no more than heal, more or less imperfectly, their injuries (*e.g.*, by scars). The former are able to regenerate lost organs. Injury may be regarded as playing quite a normal part in the development of some types. Thus the grasses of our fields and lawns grow all the more vigorously and thickly for being cropped.

We are now in a position to discuss the statements "acquired characters are transmissible," and its contradiction "acquired characters are not transmissible." We have seen that *all* the individual inherits, all that he can be conceived as inheriting, is a bundle of potentialities to grow this way and that in response to

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this stimulus and that. His nature is the sum of his potentialities ; his nurture is the sum of the influences that play on him and convert his potentialities into actualities. We have seen that nothing develops, or can by any possibility develop in the individual unless nature and nurture co-operate. Lastly, we have seen that the higher animals are of such a nature that they cannot reach maturity and fitness for the struggle for existence and offspring except under the nurture of use and experience.

Now imagine two men, both of whom have developed the common characteristics of Englishmen, but of whom one is tall and dark, with hands hardened by rough usage, a skin scarred with wounds, and a mind innocent of scholarship, and the other fair and short, with the soft hands, delicate skin, and cultivated mind of the scholar. The two men are like and unlike both by nature and by nurture ; or, to use still more familiar terms, some of their likenesses and differences are inborn and some acquired. Their inborn likenesses and differences arose because their potentialities for development were in some respects similar and in some dissimilar. Their acquired likenesses and differences arose because the stimuli in response to which they developed were in some respects similar and in others dissimilar. Here the words "inborn," "acquired," and "inherit" are used intelligibly. We know clearly what they imply. Their meanings can be explained and understood. When Darwin propounded the theory of Natural Selection he reasoned that nature selects variations—i.e., innate, inborn, germinal, or natural differences. His theory as propounded by himself is also intelligible. We know clearly what he meant. It also can be explained and understood.

But the words "inborn" and "acquired" are not always used to distinguish likenesses and differences. More often they are employed to describe characters. Thus a head, a hand, a sexual trait, and an instinct are termed inborn, while a scar, a roughened skin, and a knowledge of Latin are termed acquired. But, while all biologists have assumed that there is a quality of innateness about inborn characters and a quality of acquiredness about acquired characters, no one has ever attempted to define the meanings of these words when so used, and to indicate precisely in what respects an inborn trait is more inborn and less acquired than an acquired

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trait. Again, while all biologists are agreed that inborn characters tend to be inherited, and while some biologists affirm and others deny the transmissibility of acquirements, no one has yet indicated precisely what is meant by the word "inherit" when used in this connection. The usual accompaniment of lack of clear and precise definition is loose and confused thinking.

Probably most biologists would define an inborn character as one which had its "roots in" or "its representative in" the germ-plasm, and an inherited character as one which was derived from the parent. But when we say that a child has inherited his parent's head, we do not mean that the actual head of the parent has been transferred to the child, leaving the parent derelict. We *can* mean only that the germ whence the child sprang derived from the germ whence the parent sprang the potentiality to reproduce under similar conditions (including similar stimuli) a similar head.* But if a head, or any other character, is inborn in this sense, then very clearly a scar and every other acquirement is inborn in exactly the same sense, for the potentiality to produce a scar is as much present in the germ-plasm (and is just as much a product of evolution) as the potentiality to reproduce a head. Probably, again, most biologists would define an acquirement as a change produced in an inborn character by the action of some external influence. But, beginning

* This statement is not absolutely correct. It is possible to mean something different, or, at any rate, something more. In the days when the transmission of acquirements was an article of universal belief, it was surmised that the child inherited his nature, not only by way of the germ-tract, but by way of the somatic cells, of his parent. It was thought that the latter sent out gemmules or representatives of themselves, which entered the germ-cell and so built up what Weismann later termed the germ-plasm. If such a supposition were true, then the child would, in a sense, inherit the actual head of the parent. Some part of it would be transferred to his shoulders. When, however, doubts were cast on the transmission of acquirements, when the improbability of pangenesis and the significance of the visible descent of the germ-cells down the germ-tract from a cell which had been capable of producing the parent, were realised, theories of pangenesis were generally abandoned. They became as unbelievable as any supposition which accounts in a wildly improbable way for events which are easily capable of a probable explanation. With the reservation, then, that an alternative hypothesis is conceivable, though not believable, it is correct to say that, when we declare that the child inherits his parent's head, we can mean, not that one head is derived from the other, but only that they are both offshoots from the same line of germ-plasm.

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with the first change in the germ-cell, all change, all development whatever, is due to the action of influences which have their origin external to the character that is changed. *It follows, if any character is an acquirement, all characters are acquirements.* In brief, these definitions lead inevitably to the conclusion that innateness and acquiredness do not distinguish characters; but, on the contrary, are common to all characters—the inborn element being supplied by potentiality and the acquired element by stimulus. I do not mean, of course, that there are no distinctions between characters. There are many. Some arise in response to this stimulus, some in response to that, some are present in every normal individual, some are more or less common, some are very rare, and so on. But I do insist that we make a nonsensical classification when we catalogue characters under headings of innate, acquired, inheritable, and non-inheritable. Those distinctions are actually unthinkable in relation to characters.

Many biologists write as if the words “inheritance” and “reproduction” were identical in meaning. But, obviously, inheritance is much the wider and more comprehensive term. It has more extension and less intension. It denotes more and connotes less. There may be inheritance without reproduction, but there cannot be reproduction without inheritance. Reproduction is inheritance plus production. The individual inherits the parental nature; he reproduces, with the aid of nurture, the parental characters. Sometimes a domesticated animal or cultivated plant, even when pure-bred, reproduces anew a long-lost ancestral trait. That trait had its representative in the germ-plasm.* It was latent, not

* I protest I am not responsible for this expression. The notion conveyed by it is, to me, almost as inconceivable as the notion that space has, or has not, limits. I only know, or think I know, that the germ-cell is of such a nature (has such potentialities for development) that it is capable, under fit conditions, of growing into an embryo, which is of such a nature that, under fit conditions, it is capable of growing into a foetus, and so on. I cannot conceive how a germ-cell, even if it were as big as a balloon, could hold separate and discrete representatives for all the infinite multitude of developmental possibilities of such an adaptive animal as man. However, Mendelians and others are convinced that each “inborn” character has its representative in the germ-plasm. If they are right, then I see no reason to doubt that every possible acquirement, every scar, every effect of use, every possible bit of knowledge, has its representative also. The evidence is at least as good in the one case as in the other.

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absent. The potentiality was there, but, the conditions not favouring, development did not follow. Much the same happens in the case of those "acquirements" which the parent produces but the offspring does not reproduce. In each case the potentiality is there ready to be converted into actuality by stimulus similar to that which the parent received.

If the literature of the controversy about the transmission of acquirements be examined, it will be found that the disputants regarded as acquired all characters which combined two peculiarities : (1) a divergence from an ideal type called the "normal," and (2) development in an obvious way in response to use, experience, or injury. Thus the muscular development of the normal man was supposed to be inborn, whereas the additional muscular development of the blacksmith was considered to be acquired. If the disputants had called the top of a wall (the layer of bricks last added) acquired, and the rest of the wall inborn, and had argued on that basis, their procedure would have been quite as rational ; for I am sure they would have found it as difficult to explain why they considered the last bit of muscular development (which occurred under precisely the same conditions, the same potentiality, the same kind of stimulus, as the antecedent development) as peculiarly acquired, as to explain why they regarded the last added layer of bricks as peculiarly acquired.*

* I am sure many biologists will repudiate, at the outset at any rate, the suggestion that, while the words "inborn," "acquired," and "inherit," have intelligible meanings when applied to likenesses and differences, they are quite meaningless when applied to characters. I shall have against me the weight of established usage and confirmed habits of thought. I am all the more anxious, therefore, to emphasise the point and elicit, if possible, comprehensible explanations from opponents. I think everyone will agree that we ought not in science to use words, especially words that lie at the keystone of our reasoning, unless we are prepared to indicate their meanings precisely. It is a truism that nothing in science has ever been definitely established except by the use of clear and unmistakable language. Equivocal or unmeaning terms lead only to confusion and endless controversy. Well, then, what meaning does the man, who applies the descriptive terms "inborn," "acquired," and "inherited" to characters, attach to them ? Does he mean by an inborn trait one especially innate, blastogenic, germinal ? Then he should be able to state precisely how a head, for instance, is more innate, blastogenic, germinal than a scar. As I say both head and scar have their roots in the germ-plasm, are products of evolution, and are parts of the soma. Is it not clear, therefore, that they are—both of them equally—not only

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How did it happen, then, that men like Wallace, Spencer, Romanes, and Weismann used unmeaning terms? The reason is plain in their books. Regarding all the characters of the normal individual as inborn, and every change produced by an easily observable cause as acquired, they never thought of characters as combined products of potentiality and stimulus (nature and nurture). In effect they supposed some characters were products of nature and others products of nurture. In other words, they assumed that an inborn trait was one which nothing evoked out of something, and an acquired trait one which something evoked out of nothing. Moreover, they assumed, on the one hand, that all characters tend to develop when used and to degenerate when disused, and, on the other, that the extent of this development or degeneration is always small. They had, apparently, no notion that all the characters of lower animals have little or no capacity to develop under the stimulus of use, that some of the characters (*e.g.*, hair, teeth) of high animals have absolutely no capacity to so develop, but that many of the characters of high animals, especially of the highest, have so great a capacity to develop in response to use that all, or almost all, the growth that occurs in them after birth

somatic, but germinal? Again, why is a head more inheritable than a scar? I can imagine many men I know answering, "Because a head is reproduced in the natural course of events, whereas a scar is not." If that be so, the English language is inheritable when an English child is reared in England, but not inheritable when it is reared abroad. Besides, what is meant by "natural"—the opposite of "supernatural" or the opposite of "normal"? What has the word in either meaning to do with inheritance? Sometimes we are told, for instance, of an imbecile child who inherits his defect, not from his parent, but from his grandparent. Here, obviously, at the back of the speaker's mind is the old belief that inheritance is from the soma, and it is implied that the child inherits, in some amazing way, from the soma of the grandparent, but not from that of the parent. What else can the speaker mean? In almost any book on heredity it will be found, even though the writer may have explained the improbability of inheritance through the soma, he is really founding his arguments on that belief. Such is the tyranny of misapplied or ambiguous words. Plainly it is impossible for science to develop under such conditions. Compare interpretative biology, with its inaccurate language, its unending controversies, its schools, its sects, its total lack of universally accepted truths, with mathematics, physics, astronomy, and chemistry, with their precise language and large bodies of universally accepted interpretation. As Francis Bacon said, "Men believe that reason rules over words, but it is also the case that words react and in turn use their influence on the intellect."

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is due to this cause. They did not note that only those characters that have grown in the individual through use degenerate in him through disuse. In brief, they did not realise that momentous phase in the history of living beings, the evolution of the capacity to grow in response to use and experience.

The scientific discussion of any subject that has previously attracted general interest (*e.g.*, the antiquity of man, or the relation of the sun to the earth) is usually begun by an acceptance of popular beliefs, and the use of popular words with their popular meanings. Later, perhaps much later, after accurate information has been gathered and the terms used and the thinking employed have grown more precise, the popular beliefs are often shown to be superficial and the popular term erroneous. In the present instance the public started with the idea that the parts of the child are derived from the similar parts of the parent—hence the division of characters into inherited (inborn) and acquired. Next Lamarck supposed that the acquirements of the parent tended to become inborn in the child. Next Darwin formulated the theory of Natural Selection, which is founded on the idea that inborn (*i.e.*, germinal) likenesses and differences between individuals (*e.g.*, parent and child) tend to be transmitted to offspring. The words “inborn” and “acquired” were now used in an entirely different connection and in accord with the new knowledge that inheritance is along the germ-tract, and, therefore, that nothing is inherited save potentiality.* Unfortunately

* The word “natural” has, in popular language, two distinct meanings. On the one hand, it means that which is technically expressed by “germinal.” Its proper opposite is then “acquired.” For instance, we use these words with these connotations when we say “John is by nature like his father, but has acquired a more polished manner.” On the other hand, it means common, usual, normal. Its proper opposite is then uncommon, unusual, abnormal, unnatural, accidental. Thus we speak of natural and accidental deaths, of natural and unnatural degrees of hairiness, or muscularity, of a child’s natural love of play, and of a woman’s unnatural hate of her child. Now, when we apply the word “inborn” to likenesses and differences, we use it as a synonym for “natural” in its first meaning. It has precisely the same connotation, and its proper opposite is the word “acquired.” John is innately like his father, but has acquired a more polished manner. But when we apply the word “inborn” to a character, we can mean nothing but natural with its second meaning. If we now oppose “inborn” used in this sense by “acquired,” we shift its meaning to natural with its first connotation. Confused thinking is a necessary consequence. It is just this confusion of thought that is responsible

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the fact was not recognised. To this day a biologist who affirms or admits the transmissibility of variations will often, almost in the same sentence, deny or affirm the transmissibility of "acquirements." Next Weismann and his followers questioned the transmission of acquired characters. Now we are taking a step further ; we are denying the very existence of inborn and acquired characters as such. Doubtless we shall be told that we are quibbling, or engaged in a logomachy, or something of the kind. But no one will seriously try to justify the traditional use of the words "inborn," "acquired," and "inherited." This man or that may begin the attempt, but just as surely he will grow confused or light will break on him. It is necessary, however, to demonstrate that we are not engaged on a mere contention about words.

What is the sequence of events which is imagined when an acquired character is said to be transmitted ? It is supposed that a trait (*e.g.*, a scar, an additional bit of muscular development, or an item of knowledge) which was developed in the parent in response to injury, use, or experience, is reproduced by the child, *but not in response to the same stimulus*. But, obviously, this is not inheritance in the same sense as when it is said that an "inborn character" (*e.g.*, a head) is inherited. In the latter case we can mean only that the child inherits the potentiality to reproduce the character *in the same way* as the parent reproduced it. But in this sense all offspring tend to inherit all the acquired characters of their parents. If a parent loses his leg, the child, under the same conditions, loses its leg also. If a child receives any injury similar to the injury received by its parent, it tends to reproduce the parental scar. We see every day workmen who have reproduced the rough hands, and English children who speak the language, of their parents. It follows when we use the word "inheritance" in reference to an "acquired" character and imply that it has become "inborn" in the child, we mean, not that the child has become like its parent in nature, but that it has become profoundly different. We mean not inheritance but transformation. In short, we have dropped the proper meaning of inheritance, and are using it as synonymous with

for the Lamarckian statement that acquirements are transmissible, and for the neo-Darwinian contention that acquirements are not transmissible. These statements are neither true nor untrue. They are purely nonsensical.

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reproduction. Or else we are supposing that the child inherits from the soma (the actual scar) of the parent.

We have seen that the characters which arise in response to injury, use, and experience, are particularly useful. They enable the individual to meet contingencies; they make him adaptable. But all the so-called "acquired" characters would be less useful, or quite useless, or worse than useless, if they were developed as "inborn traits." Conceive, for example, the utility of a scar to a man who has not been wounded, or of horny hands to a lady. Acquired characters are as unquestionably products of evolution as inborn characters. The Lamarckian supposes that after this prolonged evolution, after ancestors have produced, or have been capable of producing, a given character in response to a certain stimulus for millions of years, a miraculous thing happens just at the moment he observes the last descendant. This individual, instead of producing the character in the ancestral way, mutates astonishingly, and produces it in a new way with which evolution has had nothing to do. Nevertheless, neither the Lamarckian nor his opponent, the neo-Darwinian, perceives cause for surprise. Confused by the sudden shift of meaning from "inheritance" to "reproduction" they argue quite seriously. But is it possible that their controversy, which has endured for so many years, could have lasted an hour if the words "inborn," "acquired," and "inherit" had been used each with only one meaning, and that a clear and definite one? *

* Apart from its foundation in the misuse of words, this "historical controversy" is quite the most amazing episode in the history of science. I think it could not have occurred had the training of biologists been less exclusively in description. Attempts, especially modern attempts, to prove or disprove the Lamarckian hypothesis have usually taken the form of appeals to small and isolated bodies of evidence, such as that derived from the experiment of cutting off the tails of some generations of rats. But employ now the ordinary method of proving by means of which all recognised truth has been established in other sciences. Make a deductive inference of consequences followed by an appeal to the widest possible reality. If the higher animals have evolved from lower types through the "transmission of acquirements," many characters which are inborn in the former should be acquirements in the latter. We have seen that low animals of the present day (and presumably those of former times) have little or no capacity to make use-acquirements. It follows that the higher animals cannot have arisen through the "transmission" of such characters. We have seen also that the higher animals develop mainly under the influence of use and experience—that is, they are compounded mainly of acquirements. As a whole, therefore, inborn traits have undergone

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Fine examples of the confusion of thought which results from the ambiguous or unmeaning use of biological terms may be found in "modern and exact" work. We have seen that the Experimental School supposes that mutations have representatives in the germ-plasm, whereas fluctuations and acquirements have not. Indeed fluctuations, since they are "often due to conditions of the environment, to nutrition, correlation of organs, and the like" are thought of as acquirements. Now try to get an idea of what is in the mind of the so exact thinker. We find that by "inherit" is meant "reproduce" and that a mutation is supposed to be a trait which nothing evokes out of something, whereas fluctuations and acquirements are traits which something evokes out of nothing. If this is not meant it is impossible to imagine what is intended by the statement that mutations but not fluctuations have representatives in the germ-plasm. It is on the authority of thinking such as this that pulpit orators announce that Darwinism has "gone by the board."

Biometricians have conducted laborious and expensive inquiries with a view to ascertain whether certain traits (*e.g.*, morality, modesty, criminality, stupidity) are products of nature or of nurture, "bred" or "created," inborn or acquired, inheritable or noninheritable. Here again "inherit" is always used as synonymous with "reproduce," and an inborn trait is conceived as one which nothing evokes out of something, and an acquirement as one which something evokes out of nothing. As far as I know all biometricians invariably conclude that every character they study is "inborn." Their method of study necessarily drives them to that conclusion. Offspring belonging to numbers of families are examined. Their physical traits are measured, their mental traits are estimated.* As might be expected, it is found that, on the average, the offspring of the

great retrogression, and acquirements great progressions. It follows that while a supposition that inborn traits tend to be transmuted into acquirements may be maintained with some appearance of plausibility, the converse is, very evidently, untenable.

* The measuring and estimating are generally done by school teachers and others who have no special knowledge of the objects studied. It is supposed that their errors tend to cancel one another. So might an astronomer seek to estimate the distance of the earth from the sun by striking an average from the opinions of persons drawn from the general public.

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same parents resemble one another as much mentally as physically. Children born of the same parents and reared in the same environment have the likest natures and nurtures to be found in the community ; and no one has given reason to suppose that parents tend to transmit their potentialities for physical and mental growth in different degrees. All this is a matter of common knowledge and needs no demonstration. It means no more than like begets like *when* both develop under similar conditions.* Biometricians have not attempted to demonstrate it. They essay something much less commonplace. It is a very general belief that man is a specially educable animal and that his moral and intellectual traits in particular are products of mental experience. Biometricians, on the other hand, affirm that moral and intellectual traits are "inherited" characters—"inherited" in the same degree as physical characters.

"The *sameness* [of reproduction] surely involves something additional. *It involves a like heritage from the parents.* . . . We inherit our parents' tempers, our parents' conscientiousness, shyness and ability, even as we inherit their stature, forearm and span . . . does not the good home depend on the percentage of *innately*" [the italics are mine] "wise parents . . . geniality and probity and ability may be fostered indeed by home environment, and by provision of good schools and well equipped institutes for research, but their origin, like health and muscle, is deeper down than these things. They are bred, not created. That good stock breeds good stock is a commonplace of every farmer ; that the strong men and women have healthy children is widely recognised too. But we have left the moral and intellectual faculties as qualities for which we can provide amply by home environment and sound education. . . . The mentally better stock in the nation is not reproducing itself at the same rate as it did of old ; the less able, and the less energetic are more fertile than the better stocks. . . . The only remedy, if one be possible at all, is to alter the relative fertility of the good and bad stocks in the community. Let us have a census of the effective size of families among the intellectual classes . . . intelligence can be

* The terms used by biometricians are so ambiguous that often there is doubt whether a writer is trying to prove that like begets like, or whether he is trying to demonstrate that a given character is "inborn" and "inheritable," and not "acquired," as is commonly supposed. The fact that, apart from variations, like begets like is known to every errand boy and nurse girl. The notion that some characters are inborn and inheritable and some are not, depends, as I say, on a misuse of words and a consequent confusion of thought. In any case the biometric attempt is absurd.

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aided and can be trained, but no training or education can *create* it. You must breed it." *

I read a book the other day in which a biometrician (indeed, two of them) concluded on the evidence of facts culled from the *Dictionary of National Biography*, that there are in England families *innately* political, legal, clerical, medical, military, naval, and so on ; and, therefore, that the nation is differentiating into classes which are by nature unlike. It is a common biometric assumption that the class of people who dwell in slums, are, on the average "innately" inferior to the classes that dwell in such places as villas.

In brief, the biometrician, "looking round dispassionately from the calm atmosphere of Anthropology," sets himself to teach unteachable man that he is unteachable—as uneducable as any imbecile. A Quaker's child may be reared with little harm by prostitutes. An English child, reared from birth by African cannibals will present as close a likeness to his progenitors and as sharp a contrast to his protectors in mental as in physical characters. It follows that home influence, good schools and companions, preachings, teachings, precepts, examples—all means by which parents have sought to mould the minds of offspring—are more or less futile. Moral and intellectual traits are as little affected by external influences as the colour of eyes or the texture of hair. If, then, we wish to increase the sum of wisdom, morality, modesty, and other desirable traits, we must breed for them. If we wish to diminish criminality, stupidity, and other undesirable traits, we must breed them out.

At this point, it may be worth the while of the reader to pause and try to conceive any moral and intellectual trait which is not *wholly* developed in response to experience—of education in the widest sense. I am sure he will fail. Traits *are* moral or intellectual only because they *are* products of experience. A beetle, a baby, and an idiot, are non-moral and non-intellectual purely because they have not gathered experience.

An illustration will best exhibit the true inwardness of biometric method. A gardener collects seeds from a number of plants of the same species. He is careful to keep separate the seeds of the

* *The Huxley Lecture*, by Professor Karl Pearson.

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different plants. He, thus, has a number of families of seeds, which he sows in patches—a family to each patch. Within each patch the conditions are as like as possible, but the patches are in very unlike situations—one in sand, another in loam, another in clay, one in moisture, another in drought, one in good soil, another in bad, one in shade, another in the sun, one in the ditch, another on the bank, one in shelter, another exposed to every wind that blows. The plants grow up. In each family the individuals are much alike, but different families differ markedly. “Now,” says the gardener, “observe this family here in the sun. You can see how like its members are one to another. Your knowledge, however, is inexact. I myself, or my assistants, have carefully measured leaf and stem, and flower and fruit. Science is measurement. We know precisely their coefficients of correlation and their standards of deviation, not only as regards such things as flowers and leaves but also for stature, thickness, greenness, fecundity, and the like. Our knowledge is exceedingly exact. We have found that in every character the plants are equally alike. What does that indicate? Does not this sameness involve a like heritage? Observe this other family in the shade. It is very inferior, with an inferiority that extends to every individual and every character of every individual. Does not that, again, indicate a likeness of heritage? Does it not prove that the plants in this patch are by nature and inheritance inferior to those in the other. I assure you we have measured with the utmost care. Walk round and observe the other patches. What is true of the two you have seen is also true of all the rest. The individuals in each family are like as peas; but the families differ widely. Whatever the environment the offspring of the same parents show themselves equally alike in all their characters. Is it possible now, to doubt the force of heredity? Hitherto, I dare say, you have supposed that while stems and leaves are inborn, flowers and fruit are acquired. You have shared the popular delusion that the latter are products of sun and soil. You perceive your mistake. They may, indeed, be fostered by good sun and good soil; but their origin, like that of the stem and leaf, is deeper down than these things. They are bred, not created. My researches mark an epoch. As I have repeatedly informed the innately moral and intellectual people who read the

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Times, when warning them against the muddlers who do not use biometry, biology for the first time is becoming exact. Statesmen consult me ; for what is true of plants is true of other living beings. Consider the crowds of inferior wretches bred in slums, and how few, relatively speaking, are the offspring of the tall and wise inhabitants of villas."

Neither biometrician nor gardener checks his conclusions by comparing offspring with parents. Both distinguish between (but do not attempt the impossible task of defining) bred and created traits, and suppose that a character which is present in every individual of a stock is bred and inheritable, not created. Both insist that the likeness which exists between offspring that are born of the same parents and nurtured in the same way, indicates a likeness in heritage. This last is true, of course, but it is not new. As already noted, it is known to everyone that, apart from variations, offspring tend to inherit parental potentialities for development ; and, therefore, when they have the same nurture, to reproduce the parental characters. Both insist that the unlikenesses which exist between offspring that are born of different parents and nurtured in unlike ways is due, wholly, or almost wholly, to an unlikeness of heritage. Here we come on that confusion of thought which results from applying the words "bred," "created," and "inherited," to characters, and from using "inheritance" as a synonym for "reproduction." The following is the line of reasoning adopted. The individuals of a family are, on the average, alike in the same degree in all characters (*e.g.*, eye-colour, hair texture, stature, span, morality, modesty, probity, geniality). This sameness indicates a sameness of heritage. Now some characters (*e.g.*, eye-colour, span) are clearly inborn. It follows that since there is a sameness of heritage, that morality and modesty and other mental traits must be inborn and inheritable. Being inborn and inheritable—evoked by nothing out of something—they are nearly, if not quite, independent of nurture. They appear inevitably, no matter what the environment. Under different circumstances they could only to a very small extent have developed in any other way, or to any other extent. Therefore the unlikenesses between differently nurtured families are due wholly or almost wholly to unlikenesses of heritage. It follows that good homes, good schools, good soil,

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good sun, are of relatively little importance. We must fix our attention on good seed. "That is the broad result for statecraft which flows from the equality in inheritance of psychical and physical characters in man," and of leaf and flower in plants. That is modern and exact thought, and the excuse for undignified letters to newspapers which assure the public that Codlin is the friend, not Short.

All biologists have assumed that some characters are inborn and some acquired, and that the former tend to be transmitted to offspring. On these points there has been complete agreement, and, therefore, no discussion. But Lamarekians have disagreed with neo-Darwinians about the transmission of acquirements, and among the latter there has been vast discussion as to what characters are inborn and what acquired. Selectionists insist that all characters that arise through the selection of fluctuations are inborn; mutationists maintain that all save those that originate as mutations are acquired; biometricians declare that all that they happen to study are inborn. Nothing like these discussions has occurred in the history of science since Bacon taught that all suppositions must be founded on verified facts, and Newton set the example of testing them by more facts. To find a parallel we must go back to the Middle Ages, when the schoolmen assumed without question the truth of various statements found in Aristotle and in their books of Divinity, and argued interminably on that basis. The discussions of biology have been strictly pre-Baconian in type and as purely formal as those of formal logic. "All cats are kings; this is a cat; therefore it is a king." "All characters are inborn or acquired; this is a character; therefore it is inborn or acquired." In discussions about real things it is quite as necessary to make sure of the premises as of the subsequent reasoning.

We are often told that the study of heredity is in its beginnings. As a fact it has nearly reached its terminus. We are aware of what happens, though we have not learnt how it happens. We know that offspring tend, with variations, to develop in the likeness of their parents, that inheritance is by way of the germ-tract, and, therefore, that nothing but potentialities for development are transmitted. What we do not know is the intra-cellular, ultra-microscopical mechanics of the process. So we know that the Post Office conveys letters, though not exactly how it conveys them. The remaining

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"problems of herity," all those around which discussion has raged, are either not problems about real things, or they are questions for the student of evolution or the physiologist. The task of the former is to discover the antecedents (Natural Selection, or what not) of evolution. The task of the latter is to discover the stimuli that influence the development of the individual and to link up in knowledge each stimulus with the growth it causes. For example, every discovery of the function of a ductless gland is a step in this process.

Before the reader lays aside this essay finally, I must ask him to try the experiment of using the words we have discussed with what I conceive are their only legitimate meanings and applications. I am sure if he thinks of heredity and evolution in terms of potentiality and stimulus, not of characters; if he bears in mind that, in the case of all characters equally, nothing but potentiality to reproduce in response to the right stimulus is inherited, and, therefore, that nature and nurture are each impotent in the absence of the other; if he applies the words "inborn" and "acquired" not to characters but solely to likenesses and differences; and if he bears in mind that "inheritance" and "reproduction" are not synonyms, but that as the former applies only to potentialities, so the latter applies only to characters, he will find that his thoughts will gain in lucidity and scope, and that not only will he see a clear way through much of the tangle of confusion and controversy that disfigures biology, but also that this magnificent field of work is much more responsive to the careful worker than he dreamed. It may, indeed, be said with truth that all the controversies of interpretative biology have arisen and persisted merely because terms have been misused.

CURRENT RESEARCH NOTES

I.—THE BLOOD AT HIGH ALTITUDES.

THE microscopical examination of the blood of both animals and men living at high altitudes shows a considerable numerical increase of red blood corpuscles. This observation was originally made by Viault in 1890. On the Andes of Peru, at a height of 14,000 feet, he found the number of corpuscles in a unit volume of human blood rose from an average normal value of five to one of eight, a numerical increase of over 50 per cent., which was maintained during a continued residence at this height. Since each corpuscle possesses a definite quantity of blood-pigment or hæmoglobin, an absolute increase of this might also be expected.

During the last twenty years, Viault's original observation has been fully confirmed by a large number of physiologists, most of whom have been specially trained in those exact methods which are necessary for this kind of work, and it is now generally accepted that, not only does an increase in the number of corpuscles take place, but that the amount of hæmoglobin in the blood also rises.

These remarkable changes have always attracted much attention, partly because they have been regarded as evidence of a therapeutic effect of mountain air, but also because it is rarely possible to demonstrate that any changes in the environment of a man are the ascertained cause of an obvious change within his body. After residence at a high level, on returning to the sea-level, within two or three days, this increase both of corpuscles and of hæmoglobin completely disappears, and the blood is now found to possess the average normal number of corpuscles and an average amount of hæmoglobin. On reascending, the blood again shows the characteristic changes which, indeed, occur under physiological conditions, and cannot be induced, except as a temporary change, by any of the experiments which have been devised to reproduce the conditions found at high altitudes. The late Professor Miescher, of Basle, considered that the changes within the blood were able to be explained as an adaptation of the body to a diminished pressure of oxygen at high levels. At the sea-level the atmosphere, which contains 20 per cent. of oxygen, will, at the height of Mont Blanc, contain the same percentage, but, owing to the rarefaction of

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the air, the amount of this gas is only equal to an atmosphere containing about 11 per cent.

Among recent contributions to this subject are those of Otto Cohnheim, of Heidelberg, and his colleagues, while, a year ago, K. Bürker, of Tübingen, published full details of observations carried out at a height of about 6,000 feet. The late C. T. Dent, C. Slater, and myself in two successive years investigated this question of the increased corpuscular richness of the blood in various Alpine stations, at the Grimsel Hospice and the Montanvert, both of which are about 6,000 feet high. Observations were also made in the Vallot hut on Mont Blanc, during six days' residence, at a height of 14,600 feet. In agreement with the work of most other observers we found an undoubted increase in both corpuscles and hæmoglobin, and this to an amount which was quite outside the limits of any experimental error. It was possible to make a large number of observations, for, instead of directly determining the number of corpuscles by counting these in the usual way, duplicate photomicrographs were taken, and these were subsequently counted at leisure, and any pair of duplicates rejected which did not agree within 10 per cent. We were convinced that, apart from the question whether the increase in number of corpuscles and of hæmoglobin was a real or apparent phenomenon exhibited by the blood at high levels, a unit volume of blood did actually contain about 20 per cent. more corpuscles and 20 to 30 per cent. more hæmoglobin at the height of 1,400 feet than at the sea-level.

The early observations of Viault were made on men and animals who had resided for years at Morococha. Most of the subsequent work has been necessarily carried out during a comparatively short sojourn at a high level. The recent work of Otto Cohnheim and his colleagues completely negatives the idea that the blood shows any change either in the number of corpuscles or amount of hæmoglobin. During a residence of fourteen days at the Col d'Olen, which is about 8,000 feet, and at the Margherita hut on Monte Rosa at 14,000 feet, they found no increase at all in the hæmoglobin of the blood. This was true both for men and for dogs. The papers of Professor Bürker and his fellow-workers, published in 1913, give a full critical account of previous work, together with their own observations, which were carried out at the Schatzalp sanatorium, which is 1,000 feet above Davos. In Bürker's opinion all the results which have been obtained from 1900 to 1910 are to be accepted with caution. Not only are some observations contradictory of others, but the inherent defects in the different experimental procedures adopted by different observers have, as might have been expected, been a source of error. He considers that the majority of observers have greatly exaggerated the actual changes which can be noticed in the blood of those who reside at high altitudes.

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It is admitted that Bürker has introduced several methods of research by means of which the question, as to what extent a residence at a high level can be regarded as a therapeutic measure, may be decided. His observations, which may be considered as giving a complete answer, show that there is an undoubted increase both of corpuscles and hæmoglobin. In each of the three men who were the subject of experiments it was evident that there was an immediate and continued increase in the number of blood corpuscles in a unit volume of blood. In one case the increase was 11·5 per cent., in another 4·6, and in a third 4·0 per cent. During the fourth week of residence, the increase remained constant, and even a month later at a much lower level of about 900 feet, the blood still showed an increased number of corpuscles, which was as much as 15 per cent. and 5·4 per cent. more than the blood possessed prior to the month's residence on the Schatzalp. It therefore appears as if the changes which are produced are permanent for some time. An augmentation of the amount of hæmoglobin was also observed, an increase of as much as 10·7, 8·6, and 7·8 per cent., and this persisted for at least a month after leaving the Schatzalp. From Bürker's figures it may be concluded that the number of corpuscles increases a little more rapidly than the hæmoglobin. In individuals who show a well-marked reaction in their blood the absolute quantity of hæmoglobin in each corpuscle is also found to augment. Under the influence of mountain air we must conclude that changes of a very constant and definite character undoubtedly occur. In agreement with Bürker's evidence this change must be regarded not as an apparent but an actual one. Moreover, it is permanent for some weeks after a return to lower levels, and the phenomenon is one which is to be looked upon as a special adaptation of the body, in response to the diminished pressure of oxygen in the air at high altitudes.

II.—THE UTILISATION OF CELLULOSE IN FOOD.

True celluloses are carbohydrates of high molecular weight. They form the chief supporting substances in the walls of vegetable cells. Cellulose, which is ingested in large quantities by herbivorous animals, would appear in many cases to be an essential food-substance for these animals, since experiments have shown that a cellulose-free diet which is otherwise amply sufficient for nutrition is incapable of supporting life, probably in consequence of its influence in promoting the normal movements of the alimentary canal.

How far cellulose itself, considered quite apart from the closely related hemi-celluloses, which are capable of undergoing a fermentation in the body by which they become capable of absorption, is a genuine food-stuff for herbivorous animals, has been for a long time a disputed question. Horses and cattle undoubtedly can utilise the cellulose in food to a considerable extent. This substance undergoes bacterial

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fermentation in the alimentary canal. Man and other domestic animals which are not carnivorous certainly can digest cellulose, and the experiments of Scheunert have definitely settled that this is due to the action of bacteria. But it is known that the products of bacterial fermentation are of little use for nutrition, and for this reason it is probable that our knowledge of the digestion of cellulose by animals is still uncertain.

In the opinion of Hans Euler, the conversion of pure cellulose by ferments into some utilisable form, for example, glucose or grape-sugar, has not yet been demonstrated. The work of Hans Pringsheim, of Berlin, in 1912, adds somewhat to our knowledge. He confirms other observers by showing that the hemi-celluloses are converted into sugars by the action of digestive ferments present in the ingested food. True cellulose, which can readily be fermented by bacteria, never yields glucose, a sugar which is the final term of the activity of several ferments upon starch. Pringsheim's experiments indicate that cellulose in the alimentary canal is first converted to a complex sugar termed cellobiose by the action of bacteria. The utilisation of this substance is now possible, since a ferment already existing in the fodder, known as cellulase or cytase, can directly convert cellobiose into glucose. The whole value of starch in nutrition depends upon a ferment action which converts this to sugar, and in Pringsheim's opinion the celluloses in the food of herbivorous animals and man is utilised by the organism by the successive action of bacteria and unorganised ferments, the latter of which are present in the food.

G. A. BUCKMASTER.

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AN INTRODUCTION TO THE CHEMISTRY OF PLANT PRODUCTS, by
PAUL HAAS and T. G. HILL. (London : Longmans, Green & Co.,
1913.) Pp. xii + 401, Med. 8vo. Price, 7s. 6d.

The title assigned to this book by its authors does not completely indicate its scope, which includes not only an account of the chemistry of some of the most important plant products, but also discussions of the modes of formation of these substances, and of their significance in plant metabolism, or more correctly perhaps a statement of the speculations which have been advanced on these subjects, for in most cases actual knowledge of these matters scarcely exists at the present time.

The authors must have experienced great difficulty in selecting their material from the great mass of matter which has been published in the various branches of this subject. Their selection will not meet with universal approval, but it has been well done nevertheless, and any student who successfully assimilates the contents of this volume will have laid the foundations of a good knowledge of the bio-chemistry of plants.

The book is divided into nine sections, each of which deals with a particular class of substances, *e.g.*, carbohydrates, glucosides, tannins, pigments, proteins, and so on. Each section begins with a *résumé* of the chemistry of the compounds dealt with, and concludes with an account of what has been suggested in reference to their mode of formation in plants and the part they play in the plant economy.

The *résumés* of the chemistry of plant products are quite successful in conveying a trustworthy general idea of the present position of knowledge of these subjects, and the section on colloids may be quoted as a good example of how such *résumés* should be written, since it is compiled on broad lines and avoids unnecessary and confusing details. Other sections, however, contain much matter, describing the isolation and estimation of plant products, that might well have been omitted, since it is amply provided in readily accessible form elsewhere.

The authors are somewhat careless about nomenclature ; thus, in the section on fats, the term " fatty acids " is at first carefully restricted to the *saturated* acids occurring as esters in fats, but later on is used for the *mixed* acids from fats, which include *unsaturated* acids, such as those of the oleic and linoleic series. Similarly, mannite and sorbite

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are used consistently up to p. 376, where they suddenly become mannitol and sorbitol, though even here inositol and adonitol are employed in place of the conventional names ending in -ol. The study of chemistry unfortunately involves an extensive acquaintance with terms and names, and it is desirable that authors should not add to a student's difficulties in this direction by using the same term in different senses, or two names for the same thing, or by disregarding the conventional nomenclature which chemists as a body have adopted for the convenience of all who follow the science.

Some of the economic information given is not quite accurate; thus it is stated that "aqueous solutions of saponins have a marked power of retaining dissolved gases, as, for example, carbon dioxide; for this reason saponins are occasionally added to effervescent drinks, such as ginger-beer or lemonade." The makers of these products add saponin preparations in order to facilitate the formation of a "head" when the liquid is poured out, which is not quite the same thing. In justice to the purveyor of these liquids the authors should have added that brewers of alcoholic liquors are by no means innocent of the same practice. Other examples of inaccuracy that might be quoted are the statement that "chromic acid recently has come into use as a substitute (*i.e.*, for tannin) especially in the manufacture of the cheaper grades of leather" (p. 192), the description of "catechu" as "the sap of *Mimosa Catechu*" (p. 198), and the reference to catechu and kino as resins (p. 201).

The portions of these various sections relating to the formation and function of products in plants are less satisfactory than those dealing with their chemistry, but this is largely due to the unsatisfactory state of knowledge on these subjects, which in some cases consists merely of ill-considered and mutually-destructive speculations. Very little criticism of these speculations is, however, indulged in, and in connection with glucosides, nitrogen bases and tannins, the suggestion is made that the bitterness or poisonous nature of these substances may serve as a protection against herbivorous animals. It is curious that this statement constantly crops up in spite of the existence of a quite important literature proving clearly that animals do not instinctively refrain from eating bitter or poisonous plants. Such a statement is absurd in view of the fact that the occurrence in pastures of plants containing poisonous constituents is a constant source of anxiety and loss to stock-keepers in various parts of the world. "Slangkop," to quote only one example, is a plant which contains intensely bitter toxic substances and has been frequently responsible for the loss of farm animals in South Africa.

The faults referred to above are, however, all in matters of detail, and can easily be remedied in a new edition. The book is on the whole a very creditable piece of work, which may do something towards creating a place for the chemistry of plant products in the educational courses

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provided for chemical and botanical students at the universities. The science graduate who has anything but the vaguest idea of what is meant by gums, resins, tannins and other plant products is a rarity at the present time.

T. A. H.

THE PRINCIPLES AND METHODS OF GEOMETRICAL OPTICS, ESPECIALLY AS APPLIED TO THE THEORY OF OPTICAL INSTRUMENTS, by JAMES P. C. SOUTHALL, Professor of Physics in the Alabama Polytechnic Institute. (New York: The Macmillan Company, 1913.) Pp. viii + 626.

This is the second edition of Professor Southall's work, and the fact that it follows the first edition by so short an interval as four years is sufficient testimony to its attractiveness and utility. Indeed, before its appearance there existed no adequate comprehensive handbook in the English language treating optics as it should be treated, with a constant and ruling regard to its practical applications. The work of von Rohr and his collaborators remained untranslated; it is not altogether to be regretted, because a translation of even a first-rate German work is apt to be unsatisfactory owing to the scanty recognition that is often found in it of the claims of English originators. In all such matters Professor Southall is studiously fair and has taken obvious pains to be complete. His work is built upon a plan which lends itself to that end. It is not an attempt to range optics along the line of development of one master idea, it is rather a dictionary or history of all the researches of recent years arranged as far as possible in a logical nexus. This method entails a certain degree of diffuseness, but those who wish for a handbook of reference will prefer diffuseness to too much condensation. Yet it strikes the writer as being at times unprofitably diffuse, and had Professor Southall been acquainted when he wrote the first edition with Mr. Leathem's and Professor Whittaker's contributions to the series of Cambridge Tracts—an omission for which he expresses regret in the present volume—he might have learnt from them a good deal in the art of compression. The book is largely, and perhaps rightly, dominated by the works of Czapski and von Rohr, and the developments of von Seidel's theory receive full attention. The prominence which now is given to Seidel's name is probably more a matter of convenience than an expression of any exceptional merit in his work, for, as Professor Southall remarks, it is next to impossible to apply it to actual cases. The great desideratum of the subject is a theory of the same kind which would permit of application and algebraic control, and in this connection it is to be regretted that Professor Southall does not appear to be acquainted with the writings of Schwarzschild in the *Gottingen Abhandlungen*, which are among the most illuminating and original of recent years. Schwarzschild works with

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the Eikonal—which is only the Characteristic Function rechristened—and maintains a sufficient algebraic control for the purpose of regulating the design of the instruments which he discusses. Another interesting writing that might be brought to Professor Southall's notice is Mr. Conrady's method of estimating achromatism in the *Monthly Notices* of the Royal Astronomical Society, vol. 64. As the book contains an enormous number of references to isolated papers, it is inevitable that there should be some omissions, but they seem to be few. Even the short interval that has elapsed since the issue of the first edition has seen the production of a considerable body of new work, and some attempt has been made to cope with this by brief appendices to the chapters. The book represents a great advance upon anything we have previously had in the English tongue.

R. A. S.

THE EVIDENCE FOR COMMUNICATION WITH THE DEAD, by MRS. ANNA HUDE, Ph.D. (London: T. Fisher Unwin.)

This work, extending to about 850 pages octavo, purports to be a critical inquiry into the problem whether the source of certain phenomena described as "Automatic Writing," and "Trance Utterances," taking place in the presence of persons called "mediums," are attributable, partly or wholly, to the medium's own self, or to the medium's power of telling the thoughts of others, or to direct communications to the medium from "spirits" or "the dead." Seeing that the writer assumes the alleged phenomena to be proved facts, and ignores any question of trickery on the medium's part, or of inaccuracy and the "Will to Believe" on the part of those who attend, or direct, or report on these sittings, it is manifest that the matter of this book is wholly outside the field of scientific investigation. No one with the slightest insight into the methods of scientific inquiry will waste energy in guessing at solutions of problems based on postulated facts the existence of which is neither demonstrated nor even probable.

The author seems wholly ignorant of the nature of scientific research and of the meaning of the word "science." She quotes, at the outset, as an important authority on the subject, a certain professor of psychology who, she says, has gained a world-wide reputation and is the most important adversary of the "spiritualistic" conclusion concerning the phenomena in question. This Professor's "scientific" views on the subject, as summed up by our author, appear to be that "mediumistic" communications are largely due either to imagination, or to the "emergence of forgotten memories" on the part of mediums more or less deeply "entranced." While adhering, however, to this interpretation of the phenomena he has himself studied, the Professor is said to regard some other manifestations, such as many or most of those with which the present book is concerned, as not explicable in these

REVIEWS

ways, but as due to a third cause, viz., "Telepathy," which furnishes mediums with a knowledge that is not to be found in their own minds. Further still, in cases where "telepathy" appears to fail as a cause, the Professor is credited with making a wide-ranging assumption of super-normal human powers, in order to arrive at his final conclusion. Now in all this account of the Professor's opinions the "trained" scientific man puts in no appearance at all. The Professor, as reported by his encomiast, has no inkling of doubt of the genuine nature of any of the phenomena which he strives to explain by the record of his thick-coming fancies.

Nowhere in this book is any definition given of what is meant by "trance," nor, of course, is any mention made of tests being applied to the mediums: not even to the chief of the entranced ones—Mrs. Piper: in order to show whether or not she was the subject of what the reporters themselves seem to accept as "trance." But since Mrs. Piper's performances generally, and her supposed communications with the dead particularly, are evidently looked upon by our authoress as the most important of all these interesting manifestations, and, further, since Mrs. Piper is credited unhesitatingly with an unlimited power of "clairvoyance" which is assumed as absolutely necessary for the argument unless it be admitted that the communications come directly from the dead, it is perfectly clear that no one who doubts any facts taken for granted in this book can have any interest in the discussion of explanations which forms the book's main content. Should, however, any really critical inquirer undertake to study the work he will find abundant evidence in favour of the medium's information having been gained by devices with which other members of this craft, both male and female, have been long known to be familiar; and he will note what a rich field of opportunities Mrs. Piper had for preparing her answers to likely questions and for ferreting out facts concerning such persons as attended, or might attend, the sittings where her powers were exhibited.

Although this work can appeal only to the Adepts of Occultism this notice of it is placed in BEDROCK on account of the additional example it gives of the mental pathology of Psychical Researchers—the only subject of interest in this matter to the scientific student. In the several articles that have recently appeared in this Review, following on Dr. Ivor Tuckett's detailed treatment of "Psychical Researchers and the Will to Believe" in the number for July, 1912, there is ample proof of the contention that even the protagonists of this "Research" are intolerant of all questioning of the facts they allege, while entirely failing to produce any sort of demonstration in their support. In his contribution to this controversy Sir Oliver Lodge passed over all the awkward details adduced by Dr. Tuckett and took refuge in general denouncements of the attitude of his opponents. And in his address

BEDROCK

as President of the British Association last year he again affirmed his belief in "discarnate intelligences" (popularly known as "ghosts") without vouchsafing a jot of evidence in favour of this personal and gratuitous confession of faith.

The attitude of science to the discussion of the subject-matter of the book before us cannot be better expressed than in the following words of Sir Ray Lankester in his article on Sir Oliver Lodge's address, published in the *Daily Telegraph* of September 30th, last year:—

"It is not the business of science to deny or affirm 'possibilities.' It is precisely by the refusal to discuss possibilities and by being at the same time willing and anxious to receive and verify tangible demonstration of a fact, however improbable it may appear, that those whom we call 'men of science' have within the last 250 years changed the whole current of human thought and created that inestimable treasure—ever growing and developing—which is now known as 'science' or 'physical science.' These regard the mental attitude which consists in refusing to waste time and attention on mere speculations as to possibilities, and on the other hand, in demanding and searching for demonstration and verifiable evidence, as the very mainspring of science. They regard the intrusion of suppositions and beliefs as to ghostly existences, unaccompanied by the smallest attempts at demonstration, into the proceedings of an Association for the Advancement of Science, as a danger and injury to that advancement, and a flagrant opposition to the fundamental principle by adhesion to which modern science has been created."

H. BRYAN DONKIN.

CORRESPONDENCE

To the Editor of BEDROCK.

NURSERY METAPHYSICS.

SIR,—Mr. Hugh Elliot has, in his haste to misunderstand me, not noticed what I wrote about empty Space :—

“ If Space had neither bounds nor contents, it could not affect our senses ; and knowledge of it would be impossible, not merely because we should be ignorant of it, but for the reason that there would be nothing to know.”

I certainly never quoted Locke “ to the effect that Space is meaningless except in so far as it is filled by matter ” ; and Mr. Elliot’s “ absolute vacuum inside a glass vessel ” has no terrors for me, because the measure of the size of the glass vessel itself comes within my definition of “ Space.”

Accepting Mr. Elliot’s assurance that he can imagine anything, I fear that when he does so he sometimes deceives himself, as do those who talk of boundless Space full of emptiness—which is nonsense ! Even Locke himself seems to have mistaken a word (distance) for a thing, when he said “ if nothing be between two bodies they must necessarily touch ” ; and the great Sir Isaac Newton attempted to get over his difficulty about empty Space by suggesting that it might be “ the Sensorium of the Deity.” So we must not be too hard upon Mr. Elliot when he objects to my saying that empty Space is nothing at all. He and Dr. McDougall will see this in time.

SESAMY.

October 28th, 1918.

THE AGENDA LEAGUE

is an association of men and women, irrespective of class, creed, and politics, who are anxious to promote a more effective and more generally extended social service, "to get done some things which need doing and can be done."

ITS ORIGIN

is to be found in an anonymous "Open Letter to English Gentlemen," which was published in the *Hibbert Journal* in July, 1910. The letter, addressed—as its title indicates—to Englishmen of gentle birth, sought to inculcate in them that truest patriotism, the love of country which in its most essential sense means nothing less than love of countrymen ; and which is therefore best expressed by service on their behalf. Of our English fellow countrymen, the letter pointed out, there are at present roughly speaking one and a half millions who can be classed as rich, three and a half millions comfortably off, and thirty-eight millions of poor, of whom *some twelve or thirteen millions are in constant need*. It is the work of a patriot to endeavour to effect a change in the environment of the less fortunate of these. The first step in such an endeavour is the awakening of a great compassion ; the second the undertaking of personal service based on scientific methods. Such was the idea from which sprung into being, towards the close of 1910, the

AGENDA CLUB,

which, however, appealed to a wider public than that of the "Open Letter." All persons without distinction who were

THE AGENDA LEAGUE

in sympathy with its spirit and aims were eligible for membership. Its primary object was to build up a spirit of individual and civic responsibility in and through definite social and public service. This was achieved by the preaching of the Agenda ideal, and by the performance of active social work both by the Club as a whole and by its members working as individuals. The work accomplished by the Club included an investigation into the condition of golf caddies (published under the title of "The Rough and the Fairway," by Heine-mann, at 2s. 6d.), the celebration throughout England of a National Health Week in 1912 and 1913, the publication of a monthly calendar dealing with social subjects, etc., etc. In September, 1913, however, it was felt by several members of the Board of Control of the Club that, for several reasons mainly concerned with organisation, its continued existence was no longer necessary. Accordingly, at a general meeting, held on October 30th, 1913, the Agenda Club was dissolved, and, on the motion of the Bolton Group, the

AGENDA LEAGUE

was constituted in its place, inspired by the same ideals as its forerunner, but framed on different lines. The primary object of the League, as of the Club, is the preaching of an ideal; therefore incumbent upon all its members is the duty of

PROPAGANDA.

In this the preparing and recruiting of young people for social service plays an important part; and the League, therefore, turns its attention especially towards schools and colleges.

In order to awaken in schoolboys and girls the knowledge of social and patriotic needs, and to inspire them with the desire to play in after life a part in their fulfilment, a list of books on social subjects has been drawn up for an "Agenda

BEDROCK

Bookshelf," intended to be placed in school libraries. Those schools which already possess a Bookshelf consider it of great value ; those which have not already acquired it are earnestly advised to do so. Linked with the duty of propaganda is, in the opinion of the League, the obligation to perform, if possible, definite

PRACTICAL WORK.

Willingness to take such work is, therefore, a qualification for membership. In order to secure that it shall be as efficient as possible, an attempt is made to adapt it to local conditions by the formation, where practicable, of local groups. Each group is independent, free to pursue its own work, and to develop along the lines best suited to itself, yet each is united with the other by a common purpose and ideal. There are several of these groups scattered throughout the country, and others are in prospect of being formed both at home and in the Overseas Dominions. The two largest groups are those in London and at Bolton. The London Group has formulated schemes for pure milk distribution on economic lines, the visiting of " girls and boys " clubs (in co-operation with the Social Welfare Association), and the institution of women visitors at police courts. At Bolton the work undertaken includes the prevention of the sale of tubercular milk, the establishment of a voluntary aid council, and the organisation of play centres for school children. Individual members of groups are not obliged to take an active part in the corporate work of the group if they are already engaged on some kind of social service. Those who are unable to join local groups are registered as " isolated and corresponding members." But it is not felt that their isolation absolves them from the responsibility for undertaking definite work. Health visiting, co-operation with local branches of the C.O.S. or " Guild of Help," the stimulating of interest in municipal affairs from a

THE AGENDA LEAGUE

non-party standpoint, the establishment of "good conduct clubs" among the mentally deficient women and girls in workhouses, are among the suggestions for service offered to isolated members by the League.

Finally, it must be reiterated that the work of

THE LEAGUE

lies in the preaching and the carrying out of an ideal. All those, therefore, who are in sympathy with the Agenda ideal are urged to promote it by becoming members of the League. The sole qualification is willingness to perform social service, either by propaganda or by performing some, at first sight, more practical undertaking. There is no desire for those who are already committed to some form of social work to take upon themselves something new. The minimum annual subscription to the League is 1s., but in addition to this local groups have the power to fix subscriptions for the furtherance of their own work. Further information—also copies of the "Open Letter to English Gentlemen" and of the lists for both boys' and girls' "Agenda Bookshelves"—may be obtained from the General Honorary Secretary, Miss A. M. Taylor, 36, Warwick Road, London, S.W.

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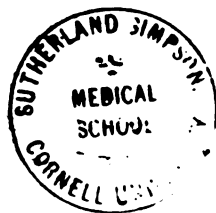
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BEDROCK

A QUARTERLY REVIEW OF SCIENTIFIC THOUGHT

NO. 1.

APRIL, 1914.

VOL. 3.

THE SIGNIFICANCE OF THE DISCOVERY AT PILTDOWN

By G. Elliot Smith, F.R.S.

WHEN Charles Darwin's *Origin of Species* appeared in 1859 his statement "Light will be thrown on the origin of man and his history" (p. 488) kindled a conflagration. Huxley's biographer tells that—

"of all the burning questions connected with that book this was the most heated—the most surrounded by prejudice and passion. To touch it was to court attack: to be exposed to endless scorn, ridicule, misrepresentation, abuse—almost to social ostracism."*

When we recall these facts it must be regarded as a happy circumstance, as remarkable as it was unexpected, that the most striking material vindication of the cause for which he suffered so much abuse and ridicule should have been furnished by Darwin's own country, and by the very county which his chief protagonist, Huxley, chose as his home during the latter years of a life spent in fighting for the truth, and in defending the reality of the former existence of just such a missing link between man and the apes as Mr. Charles Dawson has found near Piltdown.

The discovery of these remains came as a great surprise. This was not due to any difficulty in appreciating the significance of a hitherto unknown genus of the human family: but the very obviousness of the meaning of the fragments excited amazement at the recovery—and in such an unlikely spot as England—of so complete a realisation of the conclusions which had previously been reached on theoretical grounds.

* *Life and Letters of Thomas Henry Huxley*, Vol. I., p. 172.

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For these few fragments of fossil bone revealed that in Sussex there existed in the extremely remote past the representatives of an archaic group of the human family altogether distinct from any of its other known members. The brains of these "dawn-men" had already attained a lowly human status, although their faces still retained many of the distinctive traits and not a little of the uncouth brutality of the apes—an association of characters which had already been postulated in the earliest man by those who maintained the view that it was the growth of the brain that first brought an ape to man's estate.

Nor was there any doubt as to the clear light these fossils shed upon the early history of the Hominidæ. For the Piltdown man is the nearest approximation that has yet been discovered to the direct ancestor of the genus *Homo* and all of its many varieties that made their appearance in Pleistocene and more recent times.

In December, 1912,* Mr. Charles Dawson and Dr. Smith Woodward clearly explained and correctly interpreted the momentous significance of their great discovery; and nothing that has come to light since then has affected the accuracy of their general conclusions, or qualified the meaning which they attached to their work. Under these circumstances it is truly calamitous that there should have been any dust-raising "hurly-burly," to use Professor Keith's expressions,† to obscure the clear story these fossil fragments have to tell.

In another place I have set forth in detail the results ‡ of a careful scientific investigation of the nature of the evidence and its interpretation; and I had hoped at one time that I should never have been compelled to enter into any controversial writing on the points at issue. But the articles which have appeared in *BEDROCK* and other journals have altered the circumstances; for the mystification which such *ex parte* statements have produced in the minds, not merely of the educated public, but even of leading anthropologists abroad, urgently points to the necessity for a frank examination and criticism of the expressions of opinion which have excited such a distracting and befogging influence.

* At the meeting of the Geological Society, reported in the *Quarterly Journal of the Geological Society*, 1913, Vol. 69, March, p. 117.

† *BEDROCK*, January, 1914, Vol. II., p. 435.

‡ Now being prepared for publication.

SIGNIFICANCE OF DISCOVERY AT PILTDOWN

When we find the distinguished Professor of Anatomy in Strassburg, than whom among anthropologists there is, perhaps, no one distinguished for clearer insight, subscribing to the view that the "*Homo sapiens* of Piltdown," as he contemptuously calls Eoanthropus, must be put aside amongst the ancient remains that are not above suspicion,* it is surely time to utter a protest.

Moreover, when it is realised that the disturbing factor, as Professor Schwalbe so naïvely confesses, was a series of letters from Dr. Duckworth, closely reflecting from time to time the fluctuations of Professor Keith's views, it is plain where the source of all the confusion is to be found.

It is, however, not so much Professor Keith's opinions concerning the Piltdown skull which have produced all the trouble, but the way in which he has mingled his discussions of this genuinely authenticated ancient type of the human family with remarks on other remains, the claims of which to belong to a remote date most anthropologists, rightly or wrongly, regard as spurious. Hence one cannot refrain from expressing regret that the attempt should have been made to use a discovery of such vast importance as that which rewarded Mr. Dawson's watchfulness at Piltdown to bolster up rash speculations based upon the imagined antiquity of the Galley Hill and Ipswich skeletons. For, whatever view we may hold with regard to the latter two sets of human remains, there is no doubt that the claims set up for referring them to a pre-Mousterian age are viewed with the gravest suspicion; and when the discussion of the Piltdown remains is obscured by frequent references to what M. Boule calls such *bric-à-brac*, it is not surprising if foreign anthropologists, like Professor Schwalbe, evidently confused by Dr. Duckworth's chameleonic adaptations to the varying colours of Professor Keith's opinions, put the Piltdown remains into the same category as the Galley Hill skeleton.

In BEDROCK,† Professor Keith once more resorts to the now familiar line of argument in reference to the Galley Hill and Ipswich men, and what he considers the wholly unreasonable attitude of Dr. Smith Woodward and Dr. Hrdlička, amongst others, in not

* *Zeitschrift für Morphologie und Anthropologie*, February, 1914, p. 604.

† January, 1914, Vol. II., p. 438.

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at once admitting the claims to extreme antiquity of human remains of modern type when found in early Pleistocene levels. He complains that they are unfair to the Galley Hill man in not accepting his credentials without adequate proof,—the now familiar phrase is, “they sentence him before he is tried” (p. 440),—while they accept the evidence of the Neanderthal group, wherever and under whatsoever circumstances their remains are found.

A more frivolous grievance it is hard to conceive. It is the business of science relentlessly to scrutinise all the evidence it uses and not to build up vast speculations from material that will not stand the most elementary tests of stability. The Neanderthal remains have the evidence of their genuineness engraven in their very texture. The Galley Hill and Ipswich remains admittedly afford in themselves no evidence of their age; and the overwhelming improbability of their being really old, in the geological sense, can only be overridden by the most positive demonstration of their genuine antiquity. Without such proofs no scientific man should be content.

But Professor Keith writes: “I see no loophole of escape from the conclusion that these remains [Galley Hill man and the other suspicious characters] are as old as the strata under which they were found” (p. 438). Let us examine the kind of evidence that so easily satisfied him; and I cannot do so better than by a quotation from the late Mr. W. H. Sutcliffe’s excellent digest,* to which I refer all who want to read a sane statement of the whole case:—

“The Galley Hill skeleton was not seen *in situ* by any scientific man; in fact, not seen by any geologist till several years after its discovery. Two witnesses of no geological training say that the beds of gravel above were undisturbed. One of them says, ‘I was struck by the undisturbed condition of the gravel in which it was embedded.’ The other declares that it ‘projected from a matrix of clayey loam and sand.’ On the evidence of these two witnesses, who contradict one another on a point so fundamental as the character of the surrounding matrix, we are asked to believe that the gravel above was undisturbed, a point always difficult to establish.”

Is there no loophole of escape from this sort of evidence? Yet it is this upon which Professor Keith relies when he asks us to put

* “A criticism of some Modern Tendencies, in Prehistoric Anthropology,” *Memoirs and Proceedings of the Manchester Literary and Philosophical Society*, Session, 1912—13, Vol. 57, Part 2, June 24th, 1913, p. 17.

SIGNIFICANCE OF DISCOVERY AT PILTDOWN

aside all the results of careful scientific investigation in the past and to banish from our minds the tremendous inherent improbability of his contention ; and in return for these large concessions we are asked to accept such trivial guarantees !

And what of the Ipswich skeleton ? Again I quote Mr. Sutcliffe :—

“ Does Mr. Reid Moir seriously mean to contend that so fragile a thing as a human skeleton could remain closely articulated whilst the glacier which deposited the boulder clay passed over it ? . . . In neither the Galley Hill skeleton nor the Ipswich skeleton is there any internal evidence of great antiquity, whilst the evidence that they are not merely burials rests, as it has been shown, on so slight a foundation that it cannot for a moment stand against the inherent improbability of the occurrence of two complete skeletons as the sole representatives of a rare type in British Pleistocene strata ” (p. 18).

Those of my readers who are not familiar with palæontological literature will appreciate the tremendous demand which Professor Keith makes upon our credulity when Mr. Sutcliffe states, in reference to the extraordinary coincidence that the two skeletons claimed to belong to this remote age were both undisturbed :—

“ Many *thousands* of bones of animals comparable in size with man have been found in Pleistocene river deposits in England, but only one mountable skeleton is known, a hippopotamus from Barrington, near Cambridge ” (p. 16).

In the foregoing paragraphs I have referred to the controversies concerning the Galley Hill and Ipswich remains to indicate how seriously the claims of the Piltdown man have been compromised amongst foreign anthropologists when they have been interwoven with such tainted and generally discredited evidence.

But it is time I left such matters and examined the nature of the discussions directly bearing upon *Eoanthropus* itself.

The remarkable statement that appeared in the last number of *BEDROCK* is surely the most amazing of all the pronouncements upon the matter which Professor Keith has yet made. It is surprising not only for the way in which the arguments in respect of several features are marshalled *against* the conclusions at which their author ultimately arrives, but even more so for its omissions. The great controversy regarding the determination of the median plane of the skull, which loomed so large in the articles by Professor Keith in the

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Illustrated London News in August, 1913, and in *Nature* in the following October and November, is not mentioned, although the large estimate of the size of the brain (1,500 c.c.), which became so inflated as the result of the error in deciding the position of the median line, is still retained.

The fragments of the Piltdown skull that had been recovered at the time of the naming ceremony of the genus *Eoanthropus* (at the Geological Society's meeting on December 18th, 1912) consisted of parts of the cranium and of the right side of the mandible.

The problem at once presented itself whether it was possible to associate a jaw which was so markedly ape-like with a cranium equally definitely man-like as parts of one individual. Some writers maintained that such an association was impossible. But Dr. Smith Woodward pointed out that, as the jaw was furnished with teeth which were certainly human in character, though of a very primitive kind, it could not be an ape's. Moreover the fragments of the cranium, although obviously those of some primitive member of the human family, displayed many features that distinguished them from all other remains of primitive man; and when they were fitted together so as to restore the cranium, a cast of its interior was obtained which revealed a form of brain presenting more primitive and ape-like features than that of any member of the genus *Homo*.

Thus those who maintain that the jaw could not be associated with the skull are faced with the utterly improbable contention that two individuals, each representing hitherto unknown and ape-like kinds of primitive men, expired side by side on the same spot in Sussex; and that the jaw of one of them was spared without the skull, and of the other the skull without the jaw. It is of course not wholly impossible for such an astounding coincidence to have happened, but the onus of demonstrating the wholly improbable contention must surely rest with those who are rash enough seriously to make the suggestion.

At the Geological Society's meeting on December 18th, 1912, Professor Keith refused to entertain such a proposition, but he objected to "(Dr. Smith Woodward's) restoration of the chin-region of the mandible and the form of the incisor, canine and premolar teeth." *

* *Quarterly Journ. Geol. Soc.*, Vol. XIX., p. 148.

SIGNIFICANCE OF DISCOVERY AT PILTDOWN

A few months later, however, he seems to have withdrawn these objections, for he is reported * to have stated that—

“the evidence is *decidedly in favour of a simian development of the canine teeth, as the authorities who have investigated the remains have declared.* That a human form should be discovered with a large canine tooth was expected by all of those who recognised the close structural relationship between man and the great anthropoids.” †

But in July, 1913, he utterly repudiated these views and adopted a reconstruction which he showed at the International Congress of Medicine in August, when he stated that the canine tooth of *Eoanthropus* might be compared in size and form to that of a Tasmanian. In the report of his remarks on that occasion ‡ Professor Keith is credited with the statement that “*the dentition was more human than simian.*” § Although this report misrepresents the views of other speakers, there can be no doubt of the correctness of the sentence I have quoted, because Professor Keith communicated his views, and drawings to elucidate them, to the *Illustrated London News*, || and there can be no doubt, from his remarkable pictures of the jaw and teeth, that he was correctly reported when he said that “the dentition was more human than simian.”

Yet in BEDROCK ¶ Professor Keith tells us that “the jaws and teeth were more anthropoid than human in character” ! Does this mean that he repudiates his statements at the International Congress of Medicine and in his journalistic excursion of August, 1913, and returns to the views expressed at the Royal Society of Medicine ? For it will be remembered that on the latter occasion he withdrew from the position of opposition to Dr. Smith Woodward’s restoration, which he took up on December 18th, 1912.

But the fourth phase of Professor Keith’s attitude on the question of the Piltdown dentition, as expressed in the last number of BEDROCK, calls for further examination.

It will be remembered that less than three weeks after Professor

* *Proceedings of the Odontological Section of the Royal Society of Medicine* 1913, Vol. VI., p. 14.

† The italics are mine.

‡ *British Medical Journal*, August 23rd, 1913, p. 457.

§ The italics are mine.

|| August 16th, p. 245, and 23rd, p. 283.

¶ January, 1914, Vol. II., p. 452.

BEDROCK

Keith protested so vigorously at the International Congress of Medicine against Dr. Smith Woodward's restoration of the canine tooth the actual tooth was discovered (on August 30th, 1913) by Father Teilhard *; and it was found that Dr. Smith Woodward's hypothetical restoration was a wonderfully close approximation to the true form and size.

When Professor Keith claims, as he does in BEDROCK,† that Dr. Smith Woodward "gave the Piltdown race massive *projecting*‡ simian canine teeth—teeth like those of a chimpanzee,"—he is making a statement for which there is no justification whatsoever in Dr. Smith Woodward's writings (*op. cit.*, *supra*), drawings or models. Yet if the inaccurate word "projecting" be omitted from the above quotation, the whole of Professor Keith's argument (p. 447) falls to the ground.

But if, merely, for the sake of argument, we admit the validity of the singularly illogical line of reasoning (p. 447) that a canine tooth of the kind recovered is quite incompatible with the presence of an articular eminence, a deep glenoid fossa and the flat-worn molar teeth, why, after demonstrating this thesis to his own satisfaction, immediately stultify the whole argument by the confession that "even if the tooth does not belong to this particular mandible we must suppose it belongs to one of the same kind" (p. 449)? This reminds one of the saying (wasn't it Mark Twain's?): "It was not Homer who wrote the poems usually attributed to him, but another gentleman of the same name."

Professor Keith hints (on p. 448) that the tooth found is a closer approximation to his reconstruction than to Dr. Smith Woodward's. But if this is so—in other words, if the tooth is virtually an ordinary human tooth—all his arguments based upon its incompatibility with the presence of an ordinary human mode of jaw-articulation (*vide supra*) lose even the small semblance of cogency they possessed.

The reasons given in support of the statement that this tooth did not belong to the mandible are entirely frivolous. The first is that it is of a very much darker shade than the molar teeth. Everyone who has had any experience of field work in palæontology or

* See *Geological Magazine*, October, 1913, p. 433.

† *Op. cit.*, p. 447.

‡ My italics.

SIGNIFICANCE OF DISCOVERY AT PILTDOWN

anthropology knows that different parts of the same tooth or bone may present the greatest contrasts in colour. In fact, the Piltdown fragments themselves show a very wide range of difference in colour corresponding to the nature of the matrix that came into contact with and stained each area of the fossil. The second reason is that "it also shows signs of long and of hard wear," whereas the jaw is claimed to be that of a young individual because "the third molar was not completely erupted" (p. 449). Professor Keith adds: "The X-ray picture of the jaw demonstrates that fact." But the picture to which he refers is available for every reader to examine for himself; * and it affords the most positive evidence that the third molar was fully erupted. But even if, as so often happens, its eruption had been delayed in this case, the degree of "wear" of the other two molars would utterly destroy the validity of Professor Keith's assumption. Professor Underwood has shown in the memoir to which I have just referred,* as well as at the meeting of the Geological Society on December 19th, 1913, the baselessness of all Professor Keith's statements in reference to the Piltdown teeth.

At the meeting of the Geological Society on December 19th, 1913, Professor Keith vigorously criticised the association of the jaw with the skull on the ground that the jaw was not mature, whereas the cranium was that of an individual of at least thirty years of age, thereby tacitly admitting the correctness of my determination of the median plane. For my estimate of the age was based on the fact that the sagittal suture was partially obliterated, and Professor Keith had, until then, refused to admit that the median sagittal line was on the fragment at all. But in BEDROCK,† after dallying with the difficulties of associating the jaw with the skull, Professor Keith definitely commits himself to the opinion that "the evidence is in favour of the mandible and skull being parts of one individual, *but that the canine tooth belongs to another individual of the same race.*"‡ But if the only reason for refusing to associate the tooth with the jaw is the age question, i.e., because the latter is immature, why abandon this objection by associating

* *British Journal of Dental Science*, October 1st, 1913, p. 650.

† *Loc. cit.*, p. 452.

‡ The italics are mine.

BEDROCK

the jaw with an adult skull, one in fact with a partially closed sagittal suture ?

The whole argument is compounded of misstatements of the evidence and an utterly illogical and inconsistent use of the admitted facts.

Professor Keith once again misquotes Dr. Smith Woodward's statement as to the cranial capacity when he says : " Instead of measuring only 1,070 c.c. as had been announced, the brain cast displaced almost 1,200 c.c. of water " (p. 451). Dr. Smith Woodward did not say the capacity was " only 1,070 c.c." He found the fragments of a cranium in association with an ape-like jaw, and naturally expressed himself cautiously when he was about to claim that such an obviously primitive creature was already possessed of a brain comparable with that of a modern man : for a capacity of 1,070 c.c. is above the border line demarcating a normal human being's from a microcephalic idiot's skull. Dr. Smith Woodward wanted to make it clear that the brain was certainly human as regards size. The actual words he employed were :—

" the capacity of the brain-case cannot, of course, be exactly determined : but measurements both by millet-seed and by water show that it must have been *at least* * 1,070 c.c., while a consideration of the missing parts suggests that it may have been a little more. It therefore agrees closely with that of the Gibraltar skull as determined by Professor Keith." †

The original cast actually measures more than 1,100 c.c., and with the necessary slight corrections it may not be more than 1,170 c.c. But even if the maximum possible additions be made to the missing occipital, frontal and temporal areas the first estimate will still be a very much closer approximation to the truth than Professor Keith's calculation of 1,500 c.c. or more.

I strongly resent a further statement of Professor Keith's :—‡

" Now in all primitive forms—in anthropoids and primitive races of men—the right and left halves of the skull and brain are approximately symmetrical in size and form ; symmetry is a primitive character therefore to be expected in an early type of man."

* My italics.

† *Op. cit.*, *Quarterly Journal Geological Society.*, Vol. XLIX., p. 126.

‡ BEDROCK, January, 1914, Vol. II., p. 451.

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All the brain-casts of Palæolithic men hitherto described are remarkable for the obtrusive asymmetry of their occipital poles. Schaafhausen, Huxley, Boule, Anthony, Klaatsch and others have emphasised this point. Special attention has been called by many writers to the striking lack of symmetry in the brains of many primitive peoples. Two years ago * I was led to the conclusion that the development of this marked asymmetry was an integral factor in the acquisition of the characteristically human status. The reason for my special resentment of the quotation is that some months ago Professor Keith first claimed that "asymmetry of the occipital poles is ultra-modern," and in doing so claimed my authority for the argument that led up to this false conclusion,† against which I protested at the time.

But he failed to add that the material I had used to demonstrate asymmetry consisted of extremely primitive brains; and the very features I relied on to display this asymmetry were simian peculiarities, which had been retained in unequal degrees in the two hemispheres of the human brain.

In that letter to *Nature* (p. 345) Professor Keith resorted to a most extraordinary treatment of my drawing of the Piltdown brain-cast and outlines of the cranial fragments. He reproduced it with misleading additions of his own, intended to make it appear absurd.

He drew a line across my diagram at the lower corners of the two parietal fragments. Such a line is oblique, because a large piece is missing on the right side, whereas, upon the left side, the corner is merely rubbed, but otherwise is practically intact. Yet Professor Keith makes the wholly unwarrantable and misleading statement that the line was drawn "across corresponding angles of the two parietal bones"; and to justify this he has drawn the outlines of the two fragments superimposed in erroneous positions one upon the other (Fig. 2).

The peculiar characters of the jaw and of every one of the fragments of the skull show that we have to deal with a hitherto unknown type of being; so that if we proceed to build up the pieces and reconstitute the skull we are not justified in assuming that it resembled

* Presidential Address to Section H, British Association; see *Nature*, September 26th, 1912.

† *Nature*, November 20th, 1913, p. 345.

BEDROCK

either an ape's or a man's skull. In other words we have no right to prejudge the issue, but must treat it as we would any other unknown fossil animal, i.e., restore it strictly in accordance with the evidence actually supplied by the fragments themselves. Nevertheless Professor Keith begs the whole question at issue when he states : " we must proceed in the task of restoration exactly as we would if the corresponding parts of a modern skull were before us." * But the fragments themselves reveal scores of structural details that are absolutely fatal to such a course. The evidence of the fossil bones themselves is quite definite and conclusive as to the exact location of the median plane of the skull, but it is 3 cm. distant from that obtained by Professor Keith. †

It is, perhaps, only just to add that a week earlier ‡ he published another mode of reconstruction which is not so inaccurate as that of August 23rd; but on October 16th § he was still clinging to the later and more inaccurate restoration. But when I was able to demonstrate upon the fossil bones themselves the most definite evidence of the true position of the median plane, all of these results of " proceeding exactly as we would if the corresponding parts of a modern skull were before us " fell to the ground.

Throughout this controversy, while his views concerning the teeth and jaw and the mode of reconstruction of the skull were in a continuous state of flux, Professor Keith has been preaching the doctrine || that there can be only one true way of restoring a skull from a series of fragments, and that he possesses the key to unlock the mysteries of skull-building. But surely he has given too many hostages to fortune, in the variety of modes of reconstruction he has championed during the last few months, seriously to convince us of the merits of his system !

In BEDROCK (p. 450) he gives us a glimpse of his method in the working. He took " a fairly large modern skull " and " worked out " upon it the fragments that had been found at Piltdown. He

* BEDROCK, January, 1914, Vol. II., p. 451.

† See the *Illustrated London News*, August 23rd, 1913, pp. 282 and 283.

‡ *Illustrated London News*, August 16th, 1913, p. 245.

§ *Nature*, p. 198.

|| At the Geological Society on December 19th, 1913; and in the Presidential Address to the Royal Anthropological Institute in January last.

SIGNIFICANCE OF DISCOVERY AT PILTDOWN

“found that the Piltdown fragments were parts of larger bones than those in the skull on which (he) had worked” (p. 450). But they became larger only because his erroneous determination of the situation of the median plane had added 3 cm. to their medial extent.

At a recent meeting of the Geological Society (December 19th, 1913) I showed by large series of comparative measurements the small size of the Piltdown left parietal—the only bone sufficiently nearly complete to be measured for this purpose.

At the meeting of the Geological Society in December, 1912, when Professor Keith “had no fault to find with the reconstruction,” in other words, admitted the diminutive size of the brain, he expressed the opinion that the being who could fashion flint implements was surely also able to speak—a remark which called forth Professor Boyd Dawkins’ curious retort that “there was no connexion between the faculty of speech and the capacity of making implements.” *

As a further instance of the curious perversity which has characterised his fluctuating attitude throughout this controversy, now that Professor Keith has persuaded himself that the Piltdown individual had a brain “rather above the modern average in size” † he denies him (or I think it probably would be more correct to say “her”) the power of speech (p. 452) !

What use this unfortunate aphasic creature made of her huge and “ultra-modern” brain (Keith) we are not told.

Professor Keith bases his statement that *Eoanthropus* could not speak upon the peculiarly ape-like character of the chin region of the jaw. But the fallacy of this line of argument has been repeatedly exposed during the last fifty years. Did not Charles Darwin himself remark that the parrot could produce articulate sounds closely imitating human speech, although its jaw and tongue present the most pronounced contrast to those of man ?

But surely it is wholly illogical to infer from the characters of the jaw that *Eoanthropus* was unable to enunciate articulate speech, even if, for the sake of argument, we admit the validity of the

* *Quarterly Journal Geological Society*, 1913, p. 149.

† *BEDROCK*, January, 1914, Vol. II., p. 451.

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contention that "all the essential features of the human lower jaw (are) adaptations to the faculty of speech." *

Human speech, as we understand the term, is obviously the result of a long evolution. When the first human beings added to the instinctive vocal expressions of their feelings and emotions a vocabulary of other sounds that each individual learned to make, for the purpose of conveying to his fellows some meaning which the members of their group had agreed arbitrarily to attach to those particular sounds, the mobile simian tongue and lips and the highly cultured motor cortex controlling their movements—all directly inherited from their anthropoid ancestors—were quite competent to produce the small variety of sounds which were needed at first. As the number and variety of words gradually increased the need for a more complex series of lingual movements would become insistent; and in time the biological value of speech in the struggle for existence would make itself felt, so as to produce an increasing specialisation of the tongue and the other muscles of articulation. When this second phase had advanced to a stage appreciably to modify the size, flexibility and mode of action of the tongue, changes would begin to manifest themselves in the jaws.

In other words, speech in the crude form must have come first; then the higher specialisation of the muscles of articulation; and finally the changes in the bones, made necessary by this development of the muscles. Speech must have existed for long ages before the bony modifications became apparent; and it is, therefore, wholly illogical and unwarranted to deny the power of speech to a primitive genus of man simply because his jaw has not lost its simian character.

In my opinion the acquisition of speech, like the development of right handedness, was incidental to, and a necessary factor in, the advancement of an ape to human rank. The earliest members of the family Hominidæ must already have acquired a much more precise means of intercommunication by means of vocal sounds than any other creature possessed at the time of the birth of humanity.

Professor Keith has so frequently repeated the statement that the

* BEDROCK, January, 1914, Vol. II., p. 446.

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remains of *Eoanthropus* "were found in the same stratum as the remains of animals which lived during the Pliocene epoch" * that he seems to be quite oblivious of the enormity of the *suggestio falsi* implied in such a half statement. For whereas remains of both Pliocene and Pleistocene animals were found, all of those which could with certainty be assigned to the earlier age were rolled and converted into pebbles: in other words, the Pliocene fossils had been subjected to treatment sufficient completely to have pulverised the fragments of *Eoanthropus* if they had really been exposed to it. But as the latter bear no evidence of any such rolling, and there are definite Pleistocene remains, also unrolled, in the gravel, there can be no question of the Pleistocene age of *Eoanthropus* itself.

In the final paragraph of his article in *BEDROCK* (p. 452) Professor Keith makes his naïve confession of faith in reference to the problems of human phylogeny. According to him Piltdown man is not our ancestor, because he had a simian chin-plate! Why that is fatal to his claim is not explained. Neanderthal man is not our ancestor because he had prominent eyebrow ridges.

As a means of escape from this unreal dilemma, but incidentally to obtain from it some forensic support for his belief in the antiquity of the modern type of man, Professor Keith appears to extract some consolation from a remarkable sample of casuistry, the crudity of which is only veiled by the misuse of Mendelian terminology:—

"We must presume . . . that the common ancestor of human races possessed simian eyebrow ridges and a simian chin-plate. From that common ancestry springs one form, in which the eyebrow ridges were dominant and persisted, but the chin-plate was recessive and disappeared, the combination found in Neanderthal man. In another form, the eyebrow ridges were recessive, but the chin-plate was dominant, the combination found in Piltdown man. We may presume, independently of corroborative facts (then what M. Boule calls the *bric-à-brac* is cited, with the significant omission of the Ipswich specimen!) that there was a third form evolved at the same time as the other two, one in which both eyebrow ridges and chin-plate were recessive." †

I do not think the resort to the Mendelian vocabulary in this

* *BEDROCK*, *op. cit.*, p. 441.

† *BEDROCK*, *op. cit.*, pp. 452 and 453.

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remarkable extract is likely to commend the views to the Mendelians or to disguise the nature and obvious aim of the gratuitous assumption. This attempt to bolster up the claims of the Galley Hill and Ipswich remains is no doubt ingenious, but the very resort to such arguments is surely a confession of utter bankruptcy of valid arguments. The statement that "it is unlikely that the peculiar eyebrow ridges arose independently in each genus" (p. 452) is a curious argument to employ in this connexion, that is, in support of the argument of the origin of modern man from the hypothetical phylum in which such ridges were missing. For if this is so, where did many modern men, such as Australian aboriginals and certain individuals of the so-called Alpine race derive their prominent eyebrow ridges? There is very convincing evidence that eyebrow ridges have developed independently in the different human races.

Professor Keith's attitude in regard to this problem of the eyebrow ridges is all the more surprising in view of the fact that for several years he has been insisting—in the opinion of most anatomists, with altogether undue emphasis—on the plasticity of the eyebrow ridges; and has been arguing that, as a slight disturbance of the pituitary gland can produce considerable ridges in any individual, such a factor may explain the specific differences in this respect in men and apes!

The claim that the Heidelberg jaw supplies evidence of the existence of a man of the Neanderthal type (p. 452) in early Pleistocene times is stoutly denied by Sobotta and Schwalbe. No remains of the latter type are known before Mousterian times. Nor before the close of the Mousterian period are any human remains known of modern type, the authenticity of which will stand the test of really scientific examination.

It is not valid argument to attack the eminent authorities who deny the existence of such evidence and to make the false accusation that they have claimed it to be *impossible* for man to have existed in Tertiary times, or the modern type of man in early Pleistocene times. All that scientific anthropologists claim is that there is not a scrap of evidence in support of such contentions, and that until such evidence is forthcoming, under circumstances which admit of no possible element of doubt, it is wholly unscientific

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to speculate upon the assumption that proof will come to light. It will be time enough to discuss the antiquity of the modern type of man when the remains of such people are discovered of an earlier age than that called Aurignacian by the French anthropologists.

Whatever the future may reveal regarding the species *Dawsoni*, which may be a relatively late and, perhaps, somewhat modified survivor of the genus that found refuge in Britain, it must be regarded as definitely settled, with as high a degree of probability as any question of phylogeny can be said to be settled, that the genus *Eoanthropus* represents the immediate ancestor of the genus *Homo*.

CORAL-SNAKES AND MIMICRY

By H. Gadow, F.R.S.

INTERVENTION is often a thankless task, and yet I feel drawn into the long controversy about Mimicry since it appears not unlikely that the butterflies, as witnesses on either side, may become exhausted and the verdict be one unproven. Many questions have been raised, some of them unexpected and yet relevant, which may be cleared up by the introduction of fresh witnesses. The worst service that can be rendered to the principle at stake is to support it by cases which, hitherto considered self-evident, reveal themselves as baseless when hard pressed. The great question can but gain by elimination, and I hope to show that the coral-snakes, one of the classical examples of Mimicry, are such a case, and that they bring to light some points which may be useful to one or other, or both of the chief pleading counsel; maybe that they combine to my risk, although not to my sorrow, as to cause these two brilliantly controversial experts to join would be a pleasing achievement.

Mimicry is a very reasonable and important principle if defined as the protective resemblance of an avoided animal by other kinds in the same country; or as those cases of outward resemblance between two different creatures of which one derives advantage through being mistaken by a third party for another possessed of disagreeable, noxious qualities. This definition implies that all three parties must either inhabit the same district, or must have done so whilst the likeness was being evolved, or at least that the third party had frequent access to both model and copy. Here we meet at once with a self-imposed difficulty. If it is a question of model and copy, one must needs be preformed, an example for the other to follow, to come to resemble, unconsciously of course, since the impelling cause is the third party which, in the full meaning of the term, is here the representative of Natural Selection. We cannot divest the word mimic of a secondary process. But it is not necessary that the model was as complete, or perfect, as it is

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now. It may have been worth copying when it was still in a less specialised stage. It is to be understood that these and other teleological terms as "worth copying," "imitating" stand short for the clumsy "being mistaken for," etc. If this argument is carried further, both parties may eventually be traced to an early, indifferent stage in which there was not much to choose between them so far as outward appearance was concerned. If this possibility is conceded, the question of future model and copy may resolve itself into this, that the descendants of both the dangerous M and of the harmless C varied and progressed cumulatively in several directions, M radiating along 1, 2 or 3, and C along 3, 4 or 5, until the two different bloods which happen along the 3-line came to resemble each other more and more positively, not as before merely through indifference. This would be an instance of the ubiquitous principle of Isotely, convergent or parallel development of different creatures along similar lines. It would not have been brought about, but greatly enhanced, by Natural Selection, and it would benefit both model and copy. The model because it advertises its unpleasant qualities and the copy because of its being mistaken for the other. However the benefiting of these two is not the primary point; if Natural Selection is unconscious, and works by elimination, it cannot be teleological, but in the present case it is working consciously. First in importance comes the third party, for which, during the indifferent stage of M and C, it was a toss up whether he got hold of the harmless or a dangerous species, but as soon as M could advertise itself sufficiently, the aggressor gave the benefit of the doubt also to those descendants of C who happened to develop along the same line. And all were happy except those which could not follow the same line, and consequently were still preyed upon.

This seems quite a legitimate view to take how a case of mimicry may be brought about, but it also shows that such a resemblance can be developed equally well without the interference of a third party, only it would then be a case of simple convergence instead of mimicry, the aggressor being an unnecessary incident.

The fact that the being mistaken for another similar but dangerous creature is an essential point of mimicry rules out of count all those numerous instances of great resemblance in shape and coloration between two kinds of animals which live in countries

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so distant that under no possible circumstances can they be taken one for the other. If these likenesses occurred in the same country nothing would save the respective creatures from being extolled as another example of mimicry. Since such cases do exist, they must be due to cases other than the working of that kind of selection which is held to produce mimicry, and this consideration endangers its very principle, at least its origin through the usually accepted particular process of selection.

It is further quite conceivable that almost identical features have been developed in two different countries, here in a dangerous, and there in a harmless creature, each of which enjoyed its advantages; then let us assume that these two champions of the same dress came to occupy the same country. The resemblance as such was a performed fact. Henceforth, suddenly, and not until then, one of the two kinds fulfils all the requirements of the model and the other those of the copy. "Mimicry" has suddenly sprung up fully fledged, but as a mere incident, with precisely the same effect as if it had been evolved by selection *ad hoc*.

Let me give an example of the last but one case. In Mexico lives a harmless little snake, *Streptophorus*, which, when confronted, erects the anterior portion of its body and, moreover, broadens its neck into a little oblong shield through some special working of the ribs. In this position the little thing hisses and sways to and fro. This behaviour appears ridiculous; not though in a somewhat distant relation, *Coluber corais*, likewise non-venomous but attaining a length of eight feet and rather irascible, with exactly the same tricks. A colour variety of this formidable species in Brazil is olive-brown or greenish, with a pair of jet-black bold streaks on the upper neck, producing a conspicuous V-shaped mark, which diverges when the neck is broadened out. Now, if these snakes lived in India, nothing would save them from being accused of, or being credited with, "imitating" the cobra with its spreading neck-shield, occasional "spectacles" and erect, swaying attitude! As it happens, these peculiarities are unique in the whole of America, where, besides the pit-vipers (rattlesnakes and moccasins), the only poisonous snakes belong to the genus *Elaps*.

This has become famous, through Wallace, as the classical example of a model for mimicry amongst vertebrates, because most kinds of this genus *Elaps* are "coral-snakes," being bright red with

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sharply-marked jet-black transverse rings, set off by narrow white and golden-yellow rings. The same strikingly beautiful dress is worn by a great number of American non-venomous snakes. The resemblance, or rather the effect, is so great that from the U.S.A. to Paraguay they are known as coralillas, coral-snakes. Elaps does not bite easily; many of the others are much more irascible. The Indians discriminate between them to a small extent, but mostly give them the benefit of the doubt. In some parts of tropical Mexico, for instance, the coralillas are considered quite harmless, in others as deadly; or the natives will tell you that those which live in the bush are bad, whilst those which establish themselves in the huts do no harm whatever "as they are already tame and therefore do not bite the Cristiano," i.e., man. This applies really to one of the best coral-mimics, *Coronella micropholis*, which in the hot lands likes to establish itself beneath the water tubs in the loosely constructed huts.

This case of mimicry can be presented in a most plausible way: There is the poisonous Elaps with its warning colours, ranging from south Brazil far into the U.S.A., and there is Coronella, with many species, from Canada to the Equator. And wherever these two genera overlap in their wide range, there is some harmless kind of snake which wears the striking scarlet coat with black, yellow or white rings, so typical of Elaps. The longest ranges are those of *E. fulvius* and *C. micropholis*, and where, towards the Equator, the latter gives way, other harmless genera and other kinds of Elaps continue that uniform.

All this is strictly true and yet woefully misleading. I myself believed in this classical case as a matter of course until experience in Mexico drew my especial attention to these beauties' individual and specific variations and general economy of mode of life. There are many advantages in taking these "coral-snakes" as we will call them promiscuously, as a test case. For reasons which will appear obvious further on, let us restrict the inquiry to *E. fulvius* and to the genus Coronella. From Mexico southwards there are so many other harmless genera that the inquiry would become bewildering.

First, as to geographical distribution. The genus Elaps is restricted to the American continent, with about two dozen species, of which only three live outside South America, notably the common *E. fulvius* and the Sonoran *E. euryxanthus*. The range of *E. fulvius*

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extends from Panama into the southern states of the Northern Union. The northern limit is a line drawn from between the two Carolinas along the thirty-fifth parallel to the Mississippi and thence towards El Paso del Norte. Excepting Texas, where it ascends to an elevation of about 1,000 feet above sea level, it is in the States limited to the lowlands of less than 500 feet. In Southern Mexico it approaches the 3,000-foot level. There are some statements still faithfully cropping up even in quite recent literature and made much of by certain uncritical writers, that one specimen of *Elaps* has been found many years ago on the "Upper Missouri" and several in the south-eastern corner of the State of Ohio. To account for this it has been suggested that these snakes have extended their range up the Mississippi valley. Competent authorities, for instance, those in charge of the Smithsonian collections, now discard the above statements as never corroborated in spite of much searching.

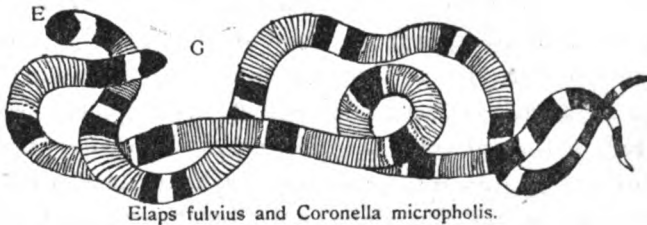
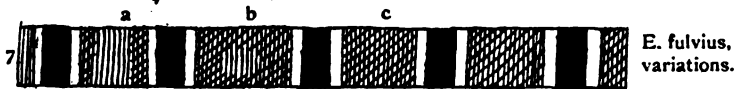
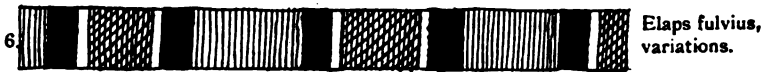
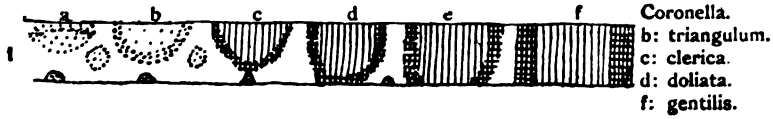
Red *Coronellas*, with black and yellow or white rings, occur in the greater part of the Union from the Pacific to the Atlantic, up to the fortieth parallel, here and there a few degrees further north, *e.g.*, in Nebraska, so that there is, roughly speaking, a belt of from 300 to 500 miles across the continent which is inhabited by non-poisonous coral-snakes to the exclusion of *Elaps*. This belt is narrowest on the east coast, say from Baltimore to South Carolina. To the north of this belt the *Coronellas* cease to be beautiful; above all they are devoid of the red colour and the transverse black rings, but such commonplace snakes occur also in the south.

The evolution of the "coral-snake pattern." Although the general effect of the coloration of *Elaps* and its supposed mimics is precisely the same for all practical purposes, there is a fundamental difference. The ground colour is red. In *Elaps* there are single black rings separated from the red by narrow yellow rings. In *Coronella* the black rings stand in pairs, separated by a yellow ring; whilst therefore black and red are always neighbours, red and yellow never meet as they do in *Elaps*. The principal variations and progressive changes of these two series can be so arranged as to represent in all probability the lines of actual evolution.

The *Coronella* type, with double black rings. It begins with a longitudinal series of darker dorsal patches upon an indifferent yellowish-grey ground colour paling into whitish on the under parts. A row of irregular, smaller, dark patches extends along the sides,

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and a third near the belly. If these patches of each of these five rows became confluent, they would be separated by four



Diagrams of the Patterns of Coral-Snakes.

longitudinal pale lines (various not mimetic Coronellas). It is in the nature of such patches that if they grow or extend, the black pigment assumes a more peripheral position, forming a pronounced

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dark border around the area of the patch, which at the same time lightens up. When such a saddle-shaped patch extends over the sides of the body, it "bursts" before reaching the ventral side, and the front and rear black border appear as a pair of curved bands (*C. triangulum* ranging from Maine and Virginia to the Mississippi).

These straighten, sometimes with the help of the lateral spots, into semi-rings, ultimately into complete black rings. Within the area of the saddles appears pigment which varies from a warm brown to red (*C. clerica*) to bright red (*C. doliata*), and these areas become the red "fields" as we shall call them, and all that remains of the xanthic ground colour clears up and appears now as the white or yellow "interstitial" rings bordered in front and behind by the black rings of two neighbouring saddles. So far this continuous series is represented by *C. rhombomaculata*, *C. triangulum*, *C. clerica* (Maryland to S. Carolina), *C. doliata* (Maryland and Florida to the Mississippi), and *C. gentilis* in Texas (diagram 1, a—f).

Further changes are introduced by progressive melanism. Black pigment appears like dust particles upon the red areas; sometimes each scale acquires a dark tip, or the black increases to specks and spots which become confluent until eventually the whole red field is transformed into a black one. The result is triads of black: short-long-short and then one or more red, non-affected fields (e.g., individual variation of *C. micropholis*, a Mexican specimen in which the process is arrested half way!)* (diagram, Fig. 3).

Another kind of change is the squeezing out of the interstitial yellow by the early approach and confluence from above downwards of the black borders of neighbouring saddles into one, now impaired black ring (diagram, Fig. 2). Besides these melanistic tendencies, there occurs, also in *C. micropholis*, the opposite, namely a growing predominance of red. Sometimes all the yellow is supplanted by red, or the red fields are longer, the black rings smaller, sometimes broken up into fragments, eventually reduced to a few irregular dorsal spots, and the whole snake, but for its head, which under any circumstances is black with a conspicuous yellow cross-band, is then almost entirely brick-red (diagram, Fig. 2, b).

The Elaps type, with single black rings, is referable to a reddish-yellow-white ground colour in which black is restricted to lateral-

* If all fields are converted, the snake becomes black, with equidistant single yellow rings.

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ventral patches of black. This black, extending upwards into the pale interstices, divides each of them, and itself becomes an unpaired ring. These black rings are not homologous with those of the border-black of the *Coronella* type (diagram, Fig. 4). Frequently the red areas are dusted or speckled with black, which pigment increases with predilection on the anterior and posterior ends of each field, producing there, against the yellow, secondary black rings. The result is narrow black triads superficially resembling those of *Coronella*. When this blackening attacks every red field, and spreads over it, the whole snake becomes black, with double white rings (individual var. of *E. fulvius*, diagram, Fig. 7).

But this same species has another, simpler and yet more complicated mode of producing triads : every alternate red field becomes black, whilst the other alternate fields remain red and absorb the narrow yellow borders. This beautiful and complicated pattern is typical of *E. surinamensis* of South America ; it occurs also individually in *E. fulvius*, as proved by a specimen which I was lucky enough to find near the Jorullo volcano. Such a triad of three black rings, cut up by two yellow rings, and enclosed in front and behind by red fields, is undistinguishable from one of *Coronella*, and yet all the components are not the same (cf. diagram, Fig. 6).

Lastly, *Elaps* also varies in the red direction ; pale rings become narrow and vanish first, next the black rings become narrow, or are broken up into irregular remnants (diagram, Fig. 4, g).

The two types described above are not restricted to *Elaps* and *Coronella*, but reoccur in a surprising number of other harmless American snakes of different groups, the numbers increasing from Northern Mexico into the tropics. Of quite innocuous aglyphous colubrines, besides *Coronella*, the following are especially worth noting : the closely allied *Cemophora coccinea* of Florida and neighbouring states, *Rhinochilus lecontei* from California to Texas, and above all the tropical *Urotheca*. Of opisthoglyphous Colubrines (with poison fangs which stand so far back in the jaws that they are effective only during deglutition, the poison being, moreover, weak) *Erythrolamprus aesculapii* appears in many combinations. In fact this and *Urotheca elapoides* sometimes hit off even the most characteristic garbs of *Elaps*.

Result : there exist in America, against some twenty species

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of Elaps, not less than eleven genera with about twenty-five species of harmless snakes in coral-snake dress. Many of them are given to great variation, both local and individual, and significantly the amplitude of these variations is greatest in those species which have a very wide geographical range, since this implies a greater variety of environmental conditions. Thus it has come to pass that almost any pattern finds its match in several kinds of snakes. If the coincidence happens in the same district and comprises an Elaps, it is extolled as mimicry ; if confined to harmless kinds it is dismissed as convergence ; and if the respective variations crop up in distant countries they are passed over in silence.

Every kind of these " coral-snakes " has a pattern which may be called normal or typical, in opposition to side-issues or exaggerated variations, which are often local although cropping up repeatedly, and comparatively rare. We can also speak of the coral-snake pattern as represented by *Coronella micropholis* or *Elaps fulvius* in its simplest form, and this being the commonest, the chances are that it is this coloration to which belong most cases of resemblance between Elaps and the harmless snakes in a given district. But an unusual variation finds its local match much more frequently in some other harmless kind, whilst the local Elaps wears another pattern.

If Natural Selection is all powerful and has brought about these resemblances, it may well be asked why it counterfeits the wrong dress. The snakes of the Gulf States are an excellent example. Whilst the Coronellas and Cemophora are brilliantly coloured, crimson or scarlet, Elaps, especially in Texas, has a strong melanistic tendency, the crimson fields being so suffused with spots and blotches of black as to impart a decidedly dull appearance, or the black rings are just as broad as, even broader than, the shortened red, much speckled or dusted, fields. They are fitly described as black snakes with broad red and narrow yellow rings. *C. gentilis* changes in Texas imperceptibly into the essentially Mexican *C. micropholis*, and this supposed champion match of Elaps, when in the border lands, actually diverges from the Texan poisonous model by very long and bright red fields with narrow black and still narrower yellow rings !

It was necessary to go into so much detail in order to show that

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the apparently endless variations of coloration are ruled by but a few principles and are, therefore, after all, limited in number. Every first stage brings in its wake a predetermined train of further changes until a terminus is reached, for instance the complete conversion of a red into a black field. Not every species reaches this end ; some stop at the beginning, others stop half way, behaving exactly like the individuals of one species ; and, what is still more important, the numerous segments of the body often represent successive evolutionary stages, thanks to the pattern being repeated on the long body from the head towards the tail. Such a snake may be equivalent to a box full of butterflies, nay superior, since all the variations are those of one individual and present at the same time. Often there is a red patch within a blackening field, here on the right, there on the left side ; or two rings have not joined completely, one being crooked. Such freaks look exactly as if they had been forgotten to be finished, and so they are, by the organism. Forgotten since they do not matter in the least. The practical effect remains the same whether a red snake has fourteen, fifteen, or sixteen black rings on its body ; it may begin to matter when they increase so much in number and width that the proportions of the colours are so seriously altered as to become obvious to the enemy or prey ; whether the organism is thereby affected is another question, which has nothing to do with mimicry. The causes of the variations are constitutional and environmental. If not, why should a model vary ? And since it does, eventually so much as to produce a totally new dress, why not allow the same to the harmless snakes, which, after all, do change just as much and in countries where they have no chance of being compared with a useful model. This is shown well by the Coronellas of the Atlantic States. The series *C. triangulum*, *clerica*, *doliata* and *gentilis* represent the evolution of coloration. To say that the Elaps likeness decreases from Florida to New York amounts to putting the cart before the horse (as some Ultra-mimicrists prefer stating the case, since this would imply that the ancestral pattern of the northerners is not really primitive but has been gained by relapse, and *C. triangulum* would ultimately revert to something like the ubiquitous *C. rhombomaculata*, etc.). The resemblance increases from north to south. In the north occur only

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dull sorts ; few brilliant kinds in the transitional belt, and in the tropical countries, both dull and brilliant sorts in abundance.

It was at first a cherished canon of mimicry that model and copy must live side by side. Since too many cases of wide separation have become known, attempts have been made to whittle away the distance, or it was suggested that the copy, after having attained the likeness, has extended its range, or that the model has withdrawn. Such explanations are quite possible, but they happen to be not applicable to the Coronellas of the United States. Next, resort was taken to the ingenious notion that the onus of the imitation did of course not rest with the snakes but with their prey and enemies, which, owing to their sagacity or stupidity, mistook these snakes for each other. And further, the enemies must be roving creatures, travellers, who could spread the fame of Elaps, preferably, therefore, birds. The same line of argument applies to the butterflies ; when it came to substantiating it by facts, it required great exertions to compile a small list of birds which are tolerably frequent eaters of butterflies. With our own somewhat extensive experience in and out of the tropics of Mexico, where we have seen butterflies in their thousands frequenting especially the cattle-stained moisture of ravines and where the birds are so delightfully little shy, on the march and in camp, in swamps, forest and plain, we have never seen a bird interfering with a butterfly.

Which are the enemies of Elaps and its imitators ? Above all, the snakes themselves. They will eat whatever they can master, and curiously enough it is the harmless, but wider-mouthed, constricting Coronella which overcomes the poisonous but narrow-mouthed Elaps. But we must restrict the inquiry to those travelling creatures which extend northwards, to the critical latitude of Maryland, Illinois and Kansas. The snake-eaters among mammals are pigs, represented by the peccaries, but these do not range to the north of Texas, and there are no hedgehogs in America. Carnivores leave them alone, except the skunk, which is practically immune. Opossums would confine their attention to tree snakes. Deer are liable to trample down any kind of snake.

Of birds there is, above all, the duck-hawk or American peregrine, some buzzards and the turkey, which are efficient destroyers of

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snakes. None of them are, however, what may be called professional snake-eaters like the African secretary and the harrier eagles of the Old World. None of these few birds and beasts, few instead of a host of migrants and vagrants, are in the least afraid of tackling a snake, except for its size, and therefore no warning dress would be effective.

Most animals have their enemies and troubles, and the balance of nature is so finely adjusted that we may well say that it is not the usual, but the occasional, enemies which upset the balance and are, therefore, in the long run the most insidiously dangerous. The many instances of truly poisonous creatures (toads, wasps, bees, ants, vipers) being eaten with impunity by their special enemies, only show that these have "gone one better." Such cases should not be used against the validity of Natural Selection.

From the attack of a professional snake-eater there is no appeal. But how can the lesson administered by *Elaps* upon an occasional foe have a lasting effect? The poison is so deadly. If the aggressor, say a bird, is bitten, he dies. It would be the old case of crushing the snake's head and being bitten in the heel. If the bird receives only a little poison, just enough to make it ill enough to remember, then, if it be hungry enough, it will probably do it again and again, until it is bitten badly and dies, or—acquires that immunity which we admire in buzzards and hedgehogs.

Another canon of mimicry, recently admitted to allow of exceptions, is that the model should be common and the copy be rare. The reverse is the case with *Elaps*. Each kind of its various copies may be rarer, but so far as *Elaps* is concerned, it is in every country overwhelmed by the number of species and the sum total of individuals which wear its dress. Consequently it should be *Elaps*, as the minority, which is benefited by the abundance of its mimics, and this would stultify the intentions of Natural Selection. It seems to have produced a chaotic muddle, anarchy, in tropical America, if we follow up the warning theory. Those philosophers who abhor the inheritance or transmission of acquired characters, bodily and mental, prefer the learning by individual experience to instinctive avoidance of dangerous creatures. Granted. Then the chances for occasional snake-eaters are that they gather their experience from the conspicuous mimics and, without hesitation,

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transfer their attention to the first Elaps they meet, with the results mentioned above. Well may Elaps wish to be delivered from its admiring mimics! Perhaps this unhappiness has caused it to vary away from the copyists, like the Texan variety! Selection must help it to do this, and, of course, make the crowd of humbugs to follow suit, the executive agents being the same kinds of birds and mammals.

If the model, Elaps, were in the overwhelming majority, then let us assume that in a given district an annual toll of 100 is required to teach the young would-be foes to leave the pretty snakes alone. If, however, the mimics are in, say, a five-fold majority, then, according to the armchair mathematician, some 600 attacks would have to be made. In reality a much greater sacrifice all round would be required, since, if only every sixth attack is a mistake and punished, the desired lesson is hugely delayed. At least, that is human nature. Therefore it is Elaps which suffers from the increased annual toll. Is there no remedy for it, no appeal to Natural Selection, or is this to work only for the humbugs?

Let us now study this process in the northern transitional belt. There are several possibilities. Elaps reigned supreme in its warning glory in the south where birds had learnt to fear it, and by giving the doubt to accidental incipient resemblance, raised harmless snakes to mimics. Spreading north, the birds still remember and teach the snakes the advantages of the new dress to come. But whilst spreading farther and farther north their memory becomes dim and the snakes remain unchanged. Perhaps this is too frivolous; let us, therefore, resort to seasonal migrant birds, which turn up suddenly in the belt, and, with their full recollection of Elaps, control the incipient variations of the harmless snakes. But surely their children, reared where they never experience a bad bite, learn to know the red-black-yellow snakes to be perfectly harmless and play havoc amongst them, their lawful prey. Next, when wintering in the south, a long process it must be for them to learn that this same dress is there sometimes connected with unpleasant consequences, only occasionally, because of the host of innocuous mimics.

Is it not more reasonable to assume that *Coronella* lived, happily balanced, in the north, multiplying and therefore spreading south,

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experiencing environmental changes which started and favoured certain variations until these happened to produce that strikingly beautiful dress? They were eaten as before, until they got so far south as to meet Elaps, when, no doubt, to their agreeable surprise, they found themselves now and then shunned like the plague.

But is this Elaps-dress a case of warning coloration? The combination of black with red or yellow is so often associated with unpleasant properties that we have fallen into the vicious circle of taking it for granted either that the bearer is bad, or if he is clearly harmless, that he must be a mimic.

There are many instances of strong colours which are not "intended" to be seen, since the animals live under ground, in the dark. Some are beautifully iridescent. Some of the most brilliant red and black, sharply marked snakes are the South American and Indo-Malayan Ilysiids and Uropeltids, harmless burrowers, some of which hardly ever seem to come to the surface. Elaps itself is eminently nocturnal and leads a concealed life. Like most of its mimics it shows a predilection for dwelling in ant-hills; so much, indeed, that Mexicans call them all *hormigueras* (anteaters), a misnomer. The red and yellow of all these snakes are not fast colours but bleach out soon in preserved specimens. They are colours which can be produced in the dark only. Further, if we take a census of all the snakes the world over, which possess some red or yellow, these colours are most frequently found on the ventral, non-exposed side. All these pigment colours are due to metabolism; they are constitutional, and their place of position also depends upon the build of the organism. Next comes the question whether these by-products, if they cannot be got rid of, be harmful, and if so, where to stow them away. If they are useful from a chemico-physical point of view, all the better. If they are visible to other creatures, that is an incident which next has to be dealt with. The effect of this visibility and pattern stands in the last line for consideration.

I have had cast models made and painted in their proper colours. They are so good that an old peacock, who, like his father, had never seen a snake, walked up to such a toy and dealt it a sharp blow with its beak, just behind the head. His eight-months youngsters craned their necks and would not go near, but some tiny bantams came up from behind and pecked at the tip of the tails! These

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models attract attention and are well visible at a distance of forty yards, in sunshine, on a lawn bank or on a border. Thrown into a flower border, the red still gives them away, but those which have fallen under shrubs into the shade are easily passed unnoticed at a few yards and are discovered only by the yellow or white rings on the black tail. Now let us put them upon gravel, a cemented floor or other grey to brownish surface and inspect them in fading light just enough to read a paper with difficulty : the most brightly tricoloured snakes seem to have vanished. With light still fading the red-black models become blurred ; those which are rather dark give themselves away by their own shadow. Monochrome black or red models keep on longer still, and longest of all, the white or yellow ringed tails. In short, the beautiful dress is not "intended" to be seen, having a most effactive, concealing effect. My attention was drawn to this vanishing trick by coral-snakes which I had laid away in camp and, later, to my alarm, could not find at nightfall.

But what about the tail ? Leaving aside the æsthetic question of beauty and that of "somatolysis" (the cutting-up of the contour, destruction of self-shadow, etc.) certain loud patterns have a dazzling or startling effect, which may be defensive or aggressive, ultimately concealing, but in a peculiarly roundabout way. Such *startling colours* are bright, and the pattern is sharply marked, the useful effect lying in the fact that the attention of foe or prey is attracted either by the momentary display of the colour (*flash-colours*, e.g., the blue or red under wings of various grasshoppers, the usually concealed yellow parts of certain frogs which are visible only during the jump) or the attention is directed away from the head to some other part, as, for instance, to the twitching tail of snakes and stalking mammals, to the cobra's shield, etc. The pennon of the lancer's weapon withdraws attention from the spike, which reaches some eighteen inches further home. A tail visible in the dusk may be useful. Concerning the body, it would of course be better to have two strings to the bow ; the perfect coloration would be one which effaces in the dark and warns in the daylight. Basking in the sun, which nearly all snakes are fond of doing, may be the critical moment in our coral-snakes' lives, but would it not be better then to harmonise with their surroundings so as to escape notice, instead of loudly advertising themselves with the chance

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of deceiving the timid and with the certainty of being eaten by their professional enemies ?

Theorists, not satisfied even with Müllerian mimicry, seem to see in the coral-snakes a huge syndicate for mutual support, and they have discovered other "rings" in Africa and other parts of the world, where puff-adders, cobras, or other poisonous snakes form centres of rally for an envious crowd. They forget that such syndicalism with a vengeance defeats its own ends. Why not declare all snakes as one great combine ? To the majority of ourselves and fellow creatures a snake is a snake, and since about every sixth kind is poisonous, the whole lot of them are feared ; but who would call this mimicry ?

THE EVOLUTION OF MIMETIC RESEMBLANCE

By Professor E. B. Poulton, F.R.S.

IN my last article in the October number of *BEDROCK* I found it necessary to devote the greater part of the available space to an issue that had been raised by Professor Punnett—the inheritance of small variations. This was an issue so tremendous that the immediate controversy on Mimicry became by comparison insignificant, and I had the less hesitation in cutting it short because I had already written elsewhere on the questions put by Professor Punnett. He now suggests that some of these questions were unanswered in my last article because I had no answer to give. I will, therefore, put his points *seriatim* in the forefront of the present article, summarising under each the answers that have already been given and including new evidence when such is available.

(1) The theory of mimicry “confers upon minute variations a selective value which is inconceivable when regard is had to the nature of the selecting agent.” To most naturalists it is not only conceivable, but even certain, that many kinds of birds can see as well as or better than man. I have published some evidence on this subject in the *Proceedings of the Entomological Society of London* (1912, pp. liii—lv). That the sight of man can easily appreciate the “minute variations” alluded to by Professor Punnett is proved by an example published on p. cxxxviii of the same *Proceedings*. The evidence is so interesting that I will quote it in full. Dr. G. D. H. Carpenter wrote to me, September 21st, 1912, from Bugalla, one of the Sesse Islands, in the north-west of the Victoria Nyanza :—

“ I caught a very nice *initial variety* of *Ps. terra* the other day. It had a very slight yellow suffusion of the black ground-colour

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along the costal margin of the forewing, and the black bar between the sub-apical and hind-marginal tawny areas was slightly thinned away. This specimen, however, *looked distinctly different*, both at rest and on the wing, which tends, I think, to show how the smallest variations may have selective value. This is always rather a stumbling block, so it was nice to see it actually exemplified."

Dr. Carpenter alludes to this and other small variations of the same mimetic pattern in this journal for October, 1913 (pp. 360—1),* bringing them forward, in fact, as an answer to this very objection. The butterflies referred to by Dr. Carpenter may be studied by any naturalist in the Oxford University Museum. They are good examples of those "minute variations" which, as I believe, have provided the steps by which mimetic resemblance has been attained.

(2) The theory of mimicry "makes the sweeping assumption that such minute variations are inherited." In answer to this objection I gave, on pp. 299—300, several examples of the inheritance of small variations, most of which are passed over in silence by Professor Punnett. I asked if he believed "that 'family likeness' is hereditary, or that one element in family likeness, such as the shape of a nose or chin, is hereditary; that a voice or trick of movement or expression is hereditary?" I gave examples of such inheritance in mimetic butterflies, described and illustrated in earlier papers, and said that he had never even referred to them. In his latest paper he still neglects them. I shall have more to say about the one example, *Danaida chrysippus*, that he attempts to explain as the result of climatic influence. In the meantime there is one piece of evidence brought forward in my last paper which has so important a bearing on this very question that I venture to refer to it again. I spoke on pp. 302, 303, of the geographical changes in the females of *Acræa alciope*, showing that "in the very zone of country where, on the theory of mimicry, we should expect them to be, we meet with the earliest stage of the eastern mimic, but, so far as we know, never the finished product." These early stages were found in western, the finished product in eastern Uganda,

* All pages quoted without indication of the original source refer to BEDROCK for October, 1913.

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but accompanying the latter is a small percentage of the early stages. A single example (Fig. 13, facing p. 62 of BEDROCK for April, 1912) was even captured by Dr. Carpenter on Damba Island in the Victoria Nyanza. It is, therefore, impossible to explain the difference by an appeal to climate; for the abundant finished product and the rare early stages—representing different levels of development—fly together in the same forest patches in eastern Uganda and have often been caught on the same day. The patterns are clearly hereditary and not caused by climate; the differences are small, and together they bridge over the gap between the western female which mimics the males of western *Planemæ* and the eastern female which mimics the male of a Uganda *Planema*.

(3) The theory of mimicry "is driven to argue for an utterly unknown and mysterious process by which these minute variations can be built up into a widely different and fixed form." This objection seems to be in large part a rhetorical re-statement of the first. The "unknown and mysterious process" is Natural Selection, its agents, insect-eating enemies, chiefly and perhaps exclusively birds. That minute variations are, as a matter of fact, "built up into a widely different and fixed form" we can see for ourselves by tracing the females of *Acraea alciope* from the Semliki Valley into Eastern Uganda.

(4) The theory of mimicry "is unable to account for the absence of transitional forms when the germ-plasms of the old form and the new one are mixed." I explain the segregation that occurs by the Mendelian theory, which, I believe, as stated on pp. 309—10, has played an important part in the evolution of mimicry, and especially of those examples in which the females appear in two or more different forms. I find no difficulty in believing that the Mendelian principle operates at many successive stages in the evolution of such resemblances, and I asked Professor Punnett why he preferred to think that it can only act once in the history of a mimetic form, and why he sought to lay this hard burden on the Mendelian principle as a factor in evolution (p. 310). He made no reply.

Within the last few weeks I have received from Mr. W. A. Lamborn a family of *Papilio dardanus* bred from a captured *hippocoon* female. While Mr. Lamborn's six previous families from the same parental form, also from Southern Nigeria, yielded no females except *hippo-*

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coon, this last includes six *hippocoon* and eight *dionysus*—a strange ancestral non-mimetic form scattered in relatively small numbers along the tropical west coast. The most probable interpretation of the facts is the assumption that *dionysus* is dominant over *hippocoon* and that the male parent was a heterozygote. Mr. Lamborn exposed some of the pupæ to cold, but this does not explain the fact that all six *hippocoon* are extremely constant, while the eight *dionysus* exhibit the most remarkable variation. *Hippocoon*, on the west coast, in the presence of the predominant model *Amauris niavius*, is abundant, and presents, in spite of minute variations which are hereditary, a nearly constant pattern. *Dionysus*, without a model, is rare and excessively variable. The contrast in nature is repeated in the offspring of a single family. It is probable that results of the same kind, but even more striking, will be obtained when *trimeni* is bred on the Kikuyu Escarpment. Professor Punnett implies that, in speaking of this latter form as “specific,” he did not claim for it specific rank, but merely meant that it was fixed and definite. I am glad to know his meaning, for the passage misled me as well as other readers. *Trimeni*, however, is remarkable for its want of fixity, and especially for variations which form a transitional series towards the male-like female on the one hand and the *hippocoon* pattern on the other.

(5) The theory of mimicry “has no adequate explanation to offer for the frequent absence of mimicry in the male sex.” Wallace originally explained this absence by the probable hypothesis that mimicry is of more value to the female, and therefore more stringently selected in this sex, than in the male. Darwin argued that this hypothesis is by itself insufficient; for why should not the advantage gained by the female be transferred to the male?—“It would be some advantage, certainly no disadvantage, for the unfortunate male to enjoy an equal immunity from danger.” Darwin continued: “For my part, I should say that the female alone had happened to vary in the right manner, and that the beneficial variations had been transmitted to the same sex alone.” The answer to-day is the same as that given by Darwin and Wallace. Predominant female mimicry is due to the fact that the sex-limited colours and patterns of females are more variable than those of males, and thus more frequently supply the material for selection.

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A more stringent selection operates upon more varied material. The variations, being linked with sex, are not transferred to the male.*

(6) The theory of mimicry "leaves without any solution those numbers of cases of polymorphism where there is no question of mimicry." It is unreasonable to suggest that variations which are not mimetic ought to be explained by the theory of mimicry. Neither this theory nor the parent theory of Natural Selection explains variation. It is the other way,—hereditary variation is one chief explanation of Natural Selection and of mimicry. Although we do not know the cause, it is the fact that female butterflies are far more subject to polymorphism in colour and pattern than the males, thus supplying material upon which female mimicry may be built up.

(7) Lastly, the theory of mimicry "endows birds with powers of selective destruction which are certainly not deducible from the available evidence." This objection again is simply the first expressed in different language. I may say, however, that I have never claimed that the direct evidence warranted any such conclusion. It is hardly likely, I think, that such direct evidence will ever be forthcoming, although I hope for the best, and shall not cease to stimulate observation on this special point. We have already a large body of direct evidence that insects with warning colours are distasteful to the majority of insectivorous birds, and that procryptic species are palatable to them. We may reasonably hope for an immense increase in this evidence. There is also some evidence that enemies are misled by mimetic resemblance, and that they remember an unpleasant experience and associate it with the appearance of the object from which it was received. On these lines, too, it is reasonable to expect far more evidence. But I do not think it likely, although of course it is possible, that there will ever be available *direct* evidence of the growth or maintenance of mimetic likeness by means of selective destruction. There is already a great mass of indirect evidence which is increasing at a very rapid rate. I allude to such observations as those of Dr. G. D. H.

* This question is more fully discussed in *Darwin and the Origin*, 1909, pp. 132—9, where the above quotations from Darwin's letters are given at greater length.

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Carpenter, published in the October number of *BEDROCK* (pp. 359—60)—the fact that the mimetic forms of a polymorphic *Pseudacraea* vary more freely and run into each other more completely on islands in the Victoria Nyanza, where their models are relatively scarce, than they do on the mainland, where their models are abundant. How interesting is the comparison between these observations in Uganda and those referred to on pp. 36, 37, as made by Mr. W. A. Lamborn in Southern Nigeria, where two female forms of *Papilio dardanus* were bred in a single family ; one of them, *hippocoon*, with an abundant model on the west coast—constant ; the other, *dionysus*, an ancestral non-mimetic form—extremely variable. The same difference exists between the wild forms, as may be seen in any good collection from the west coast. No cause, except selection, has been suggested for the relative rarity and variability of the non-mimetic form as compared with the abundance and constancy of the mimetic, and the same comparison holds between *trimeni* and the fully developed mimetic forms of East Africa. Indirect evidence along these and other lines is, as I have said, accumulating steadily and rapidly, and will probably convince the great majority of naturalists.

I now propose to deal with other issues raised by Professor Punnett in his last article.

Of course I agree that " Charles Darwin's work is not beyond fair criticism any more than that of any other man." Weismann's contention that " acquired characters " are not transmitted was a criticism of Charles Darwin's work, and I endeavoured, with others, to introduce it to English zoologists. But this new contention, as it was then, had been investigated with the utmost care and was supported by evidence on the most varied lines. How utterly different is the spirit of Professor Punnett's rash and unsupported assertions. The hereditary transmission of small variations plays an infinitely more important part in the Darwinian theory of evolution than the principle against which Weismann developed his carefully planned and elaborate attack. And Professor Punnett is content to sweep the whole fabric aside without evidence, without critical examination. The dogmatic statement that the inheritance of minute variations is a " sweeping assumption " does imply that either Darwin or the speaker is a hasty generaliser ; and it is well to

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create prejudice against the attempt to settle tremendous issues in this offhand manner.

I pointed out in 1909 that Professor Bateson and Professor Punnett had misinterpreted de Vries to English readers, the former even stating that the Dutch botanist makes a "clear distinction" between "fluctuations" and "mutations"—"clear" forsooth, when the language used was so much the reverse of limpid as utterly to mislead the exponent himself! Until Professor Punnett's last article, in *BEDROCK*, published in January of the present year, I have seen neither defence nor admission of error on the part of these two exponents of de Vries. Now, however, Professor Punnett does admit that he "may have erred," but maintains that, although de Vries does not make the distinctions he had imputed to him in a popular work intended for general readers—still the distinctions were those which de Vries ought to have made! Indeed, so strongly does Professor Punnett feel this that he tells us he is going to continue to use de Vries' terms, not in the sense in which de Vries uses them, but in that which he wrongly attributed to de Vries. How useful these words will be, and what an aid to clear thinking, in the controversies of the future!

Professor Punnett supposes that I maintain de Vries' "fluctuations" to provide the variational steps by which mimicry was brought about. I stated in the October number (pp. 297, 298) that de Vries' "fluctuation" and "mutation" were the same as Galton's "regressive" and "transilient variation." The "fluctuations" or "regressives," if they exist at all, are clearly not the steps of evolution as they were imagined by Darwin or by the Darwinian to-day. Galton at one time maintained, and de Vries now maintains, that the advance which can be made by these steps soon reaches its limit. The small evolutionary steps on which Darwin relied are the very same variations which some writers would now seek to call "Mutations," as if they were something "new and strange"—variations which Weismann showed to be germinal in origin, and therefore called "blastogenic." These furnish the steps of evolution everywhere, including, of course, the production of a mimetic likeness. When selection ceases, the likeness is soon blurred and, finally, obliterated by the appearance of other germinally caused variations that are no longer eliminated.

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I now turn to Professor Punnett's argument that, because the seasonal forms of certain butterflies are capable of being evoked by certain stimuli, the small differences between sub-species or geographical races may be "acquired" by climatic influence. But the former examples were first known to be seasonal because of the times at which they appeared. The latter, too, may differ in their response to seasonal stimuli, but they also differ in other features that appear independently of climate, even when the seasons differ as greatly as in Africa.

Much of the best systematic work of the present day consists in the establishment of these very geographical races, generally distinguished by small differences, but keeping true to their locality. If sub-species are real, then minute variations must be inherited. Professor Punnett suggests that they are unreal. "It seems to me not at all unlikely that the differences are what are often vaguely termed climatic," he says of the local changes in the average size of a spot in *Danaiida chrysippus*; and he must hold the same views for all other small geographical variations if he is to maintain the position that no clear case of such inheritance has been proved to exist. I should have thought that he would have spent many years in breeding experiments before he thus ventured to sweep away the foundations of so much good work. But this is not the method of the present-day writer on evolution. Johannsen weighs beans, de Vries records the variations of Evening Primroses, and instantly, without any further effort, without even troubling to read de Vries himself accurately, the whole foundations of evolutionary thought are assumed to be broken up.

Now that the question has been raised, it will doubtless fall to my friends to make the experiments which will test whether sub-specific characters are real or unreal. Indeed, I have already written to several naturalists on the subject.

In the meantime, there are very strong reasons for rejecting Professor Punnett's suggestions that these local differences are climatic. The fine butterfly *Danaiida plexippus (archippus)*, known in North America as the "Monarch," is a close ally of *D. chrysippus*. It extends through nearly the whole of the American continent, splitting up into at least three geographical races, one in North America, two in the South. There is reason for the belief that it

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was originally an Old World butterfly, and that it reached America by way of the north. It has at any rate inhabited North America long enough to have produced an exceedingly perfect mimic, while it has wrought no such effect in the South. In spite of its long sojourn in the New World, its pattern still strongly resembles that of its Old World allies. Nevertheless, it has formed geographical races, distinguished from each other by small differences of pattern.

During the past seventy or eighty years this butterfly, probably aided by steam transport, has been spreading to many parts of the Old World, both west and east of America. Commander Walker, who has made a special study of the subject, has kindly furnished me with the dates at which it was first recorded from the following localities :—

First westward : New Zealand, 1840 ; Marquesas Islands, “ about 1860 ” ; Sandwich Islands, 1845 — 50 ; Caroline Islands, 1857 ; Tonga, 1863 ; Niuafoou, 1866 ; Samoa, 1867 ; Tonga Tabu, 1868 ; Rarotonga, 1869 ; Tahiti, 1870 ; Lord Howe Islands, 1870 ; Clarence River, N.S.W., 1871 ; Melbourne, 1872 ; Queensland, 1870 ; Solomon Islands, 1887 ; New Britain, 1895 ; Hong Kong, 1896 ; Straits of Malacca, 1889.

Next eastward : Azores, 1863 ; Canary Islands, 1893 * ; British Isles, 1876 ; France, 1877 ; Atlantic Ocean (200 to 300 miles from the British shore), 1880 ; Atlantic Ocean (sixty miles from Cape St. Vincent), date ? ; Gibraltar, 1886 ; Grecian Archipelago, 1897.

In many of these localities the butterfly has established itself, and is now apparently a permanent resident. In spite of the great climatic differences to which it has been subject in the course of this extensive colonisation, Commander Walker has never seen a record of any except the North American form. The natural inference is that the species is not sensitive to climatic conditions, and that the South American races are not due to this influence.

The eastern and western sub-species of African butterflies nearly always meet and interbreed in eastern Uganda or western British East Africa. How can climate explain the phenomena that are manifest at their overlap—either an abrupt replacement, or, probably

* The butterfly certainly reached the Canary Islands much earlier than 1893. I saw it myself in Grand Canary in 1888.

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more often, a series of transitional variations? Furthermore, in *Danaida chrysippus* itself, although there is a marked geographical difference in the average size of a certain white spot on the forewing, yet in the same locality and at the same time, these spots are seen to vary greatly in size.

Professor Punnett says that he is not prepared to subscribe without reserve to the view that the female of *Elymnias undularis* was originally like the male. He does not discuss, and is probably not aware of, the evidence—very old and well known—which makes this conclusion probable. I remember exhibiting illustrations of the following series in an evening lecture before the British Association in 1890.

A little group of Oriental species and sub-species of *Elymnias*, of which *undularis* is one, presents us with the following sequence: (1) in the Andaman Islands, both sexes alike and resembling all the other non-mimetic males of the group, including *undularis*; (2) in Sikkim and North-East India, also in Ceylon—female mimetic, male non-mimetic; (3) in Burma—female often with white hind wings in mimicry of a *Danaine* model with white hind wings, male non-mimetic; (4) in South India—female mimetic, male with a pattern intermediate between that of the female and the non-mimetic male of other localities.

Such a sequence will satisfy most naturalists that the hypothesis doubted by Professor Punnett is the only one consistent with the facts. I am very far from denying that in some cases of sexual dimorphism, the male form may be the more recent, but, so far as I am aware, in all examples with mimetic females, the evidence—whenever evidence is available—points in the same direction as that furnished by *Elymnias*. Thus the facts known concerning *Papilio polytes* will be admitted by most naturalists to support the same conclusion. The females are not, as Professor Punnett implies, constant and invariable. They vary greatly in the same locality, and still more in the different parts of their geographical range. The male pattern is far more constant, although it too undergoes recognisable geographical changes. Furthermore, it is not only more constant than the female, but it resembles the pattern of other allied species. Such resemblances between species have hitherto been accepted as evidence of descent from a common ancestor. In other

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words, patterns like those of the male *polytes* have been regarded as ancestral, as compared with their females, which, diverging in various directions, resemble the patterns of remote species. In the most eastern part of the range on the Asiatic mainland, the *aristolochiæ* models are distinguished by the small size of the white patch on the hind wing. The mimetic females follow them. In Borneo and Sumatra *aristolochiæ* is represented by *antiphus*, without any white spot. The mimetic females follow them, although a small trace of the spot is present in some individuals.

In BEDROCK for last October it was argued (p. 309) that the red submarginal spots of the mimetic females were derived from those already present on the under, and occasionally on the upper, surface of the non-mimetic male. I have lately re-studied this question with a much larger series of individuals and have found additional evidence pointing to the same conclusion. One spot in the series—that below vein 4—is nearly always much smaller than those on either side of it and sometimes it is altogether wanting when they are present. This relationship is commonly found on the under surface of the males and male-like females, and on the upper surface also, when these spots are present,—as they are far more commonly in the male-like females than in the males. The same relationship is also common on both surfaces of the *polytes* females as well as of other mimetic female forms. It is less often seen and less striking in the *hector* form (*romulus*) than in the *polytes* form from the same locality, corresponding with other evidence, based on the evolution of the pattern, which indicates that the former is further removed from the ancestral appearance of the male than is the latter.

I have never contended, as Professor Punnett asserts (p. 571), that because the difference between the patterns of the mimetic and non-mimetic females of *polytes* is "somewhat complex," it cannot have arisen as a single mutation. I grant that a large and complex variation may arise. Professor Punnett passes over the real improbability—the sudden origin of a complex pattern which matches that of another and remote species. The same difficulty is encountered by the hypothesis that the mimetic females of *Elymnias* arose suddenly.

We know that the mimetic and non-mimetic females of *polytes* are produced without intermediates, and the question is whether

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they developed by a series of stages or suddenly arose in their present form. In the course of this discussion, Professor Punnett triumphantly pointed to the admitted fact that they do so arise ! It was this mode of argument that I ventured to parody in a travesty taken too seriously by Professor Punnett.

I stated in October, 1913, that I had always recognised that the first variation which initiates mimicry must be something appreciable, and proceeded to prove it by quoting a striking example from my article in *BEDROCK* for April, 1912. I even complained, and justly, that Professor Punnett had altogether misrepresented me. The only reply that he now makes is triumphantly to assert that the views expressed and illustrated in April, 1912, were "elicited" by his article in July of the following year. I suppose he will now claim that the earliest statement of the kind I remember to have made *—in 1890—was elicited by him !

There is nothing inconsistent between these views upon the origin of mimicry and the passage quoted from *Darwin and the Origin*, by Professor Punnett. I do not regard the "first colour change" which started mimicry as a large variation, or one that differs from the steps of evolution as Darwin postulated them.

I am reminded, by Professor Punnett's particoloured rabbits, of the hooded rats figured by Professor W. E. Castle (*Heredity and Eugenics*, Chicago, 1912, p. 58). Here is a Mendelian investigator who has been led by his experiments to believe that "Mendelizing characters can be modified by selection," and I bring this article to a close by quoting part of the concluding paragraph of his lecture :—

"Accordingly we conclude that unit-characters are not unchangeable. They can be modified, and these modifications come about in more than a single way. Occasionally a unit-character is lost altogether or profoundly modified at a single step. This is mutation. But more frequent and more important, probably, are slight, scarcely noticeable modifications of unit-characters that afford a basis for a slow alteration of the race by selection. . . ."

* *Nature*, October 2nd, 1890. Reprinted in *Essays on Evolution*, 1908, p. 376.

MECHANISM *v.* VITALISM : VERDICT AND JUDGMENT

By Charles Mercier, M.D., F.R.C.P.

I TAKE off my hat to Mr. Elliot. After abortive controversies with Aristotelian logicians, suffragette surgeons, vegetarian playwrights, and other logical cripples who take refuge in silence and refuse to answer arguments merely because they are unanswerable, it is refreshing to encounter an antagonist like Mr. Elliot, who does not know when he is beaten, and refuses to abandon an untenable position.

The terminology that Mr. Elliot and Dr. McDougall have imported into this old dispute seems to me inaccurate, question-begging, and calculated to raise prejudice. Vitalism and vital force should refer in accurate use to something inherent in all living things, vegetable as well as animal, and there is no valid reason why these should be substituted for the terms Interacting Dualism and Mind, which is what they really mean in this controversy, and which are old well-established terms. Mechanism and Materialism seem to be other names for some form or variant of Psycho-physical Parallelism, though what particular form or variant of this doctrine Mr. Elliot now favours is not clear. The real crux of the controversy is, I think, this, and Mr. Elliot will correct me if I am wrong : The doctrine of Vitalism, or of Interacting Dualism, as I prefer to call it, is that Brain and Mind are two substances, hence the term Dualism, which reciprocally act and react on one another, brain-processes producing in certain circumstances changes in mind, and certain mental processes producing changes in brain. Mechanism, or Psycho-physical Parallelism, is also Dualism, but not interacting dualism. Its doctrine is that there are two substances, Brain and Mind, which do not reciprocally act and react on one another, but

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the operations of each form a closed circuit, completely separate from the operations of the other, without any power or influence in modifying the operations of the other ; but yet the two sets of operations have a certain parallelism, so that whatever operation or process is going on in the brain is mirrored or shadowed in a mental process, and *vice versa*. The whole question at issue would seem to be : Is there or is there not interaction between brain and mind ? I say this seems to be the whole question at issue, but there is some doubt whether this does not cover more than the question at issue between Dr. McDougall and Mr. Elliot. As far as I have followed the controversy, little or no allusion has been made to whether a brain process may not cause an alteration in mind, and the main, if not the whole, question between the antagonists is whether a mental operation, *i.e.*, an exertion of the will, can or cannot cause an alteration in the working of the brain. This is the issue that has to be tried.

It is desirable at this stage to correct a misconception of Mr. Elliot's as to the nature or *locus* of the problem—to the province to which it belongs. Mr. Elliot says much that implies that the problem is a problem in science, and even in physiology, and he makes great play with the words science and scientific, which in this connection, and as he uses them, are what Jeremy Bentham called question-begging epithets. Science is commonly used, and Mr. Elliot adopts this use of the word, to mean knowledge that is more certain and more accurate than other knowledge ; and scientific is used in a vague and inflated sense to convey the impression that the scientific man is less liable to err than the man who is not called scientific. Mr. Elliot calls himself a scientific materialist, and says he is defending a scientific doctrine, which few physiologists question. Now, in the first place, I deny that scientific knowledge is necessarily or *per se* more certain than much knowledge that would be called unscientific. Scientific knowledge is systematised knowledge ; unscientific knowledge is unsystematised knowledge. That is the whole difference between them ; and scientific men sometimes organise their knowledge upon an erroneous system, and then their science is erroneous. The mere fact that scientific men sometimes disagree among themselves shows that they are not infallible, and that scientific knowledge may be erroneous. On the other hand,



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the unsophisticated rustic, if Mr. Elliot will pardon me for dragging him in, knows that if he hits a brick wall with his fist he will hurt himself, and this item of knowledge, though quite unscientific, is quite as certain and as accurate as the knowledge the scientific astronomer had a few years ago that all the planets and satellites in the solar system revolve round their primaries in the same direction.

In the second place, I deny that the problem of the connection between brain and mind is a scientific problem at all ; and I deny that physiologists are any better authorities upon it than are physicists or chemists. It is a problem, not in science, but in metaphysics ; and Mr. Elliot has an inkling of this, for, in the first sentence of his first article in *BEDROCK*, he declares that the problem lies on the borderland of science and metaphysics, and subsequently in the same article speaks of this problem as "another aspect of metaphysical problems." I hold, with Mr. Elliot when he is in this mood, that the problem is purely metaphysical. As commonly understood, the problem of metaphysics may be stated thus : What is the relation of the world that we observe to the observing mind, and how far does the observing mind obtain a true picture of the world that it observes ? This is what is usually understood by the problem of metaphysics, but I hold, and I think it is manifestly true, that there is another aspect to the relation between Subject and Object. What goes on in our minds is subjective, is a series of events in the world of mind ; what goes on in our brains is objective, is a series of events in the world of matter. The relation between the two series of events is a relation between Subject and Object, and therefore is subject-matter of metaphysics.

But according to general opinion, which Mr. Elliot endorses, the problems of metaphysics are inherently and by their nature insoluble. "Scientific questions," he says, "unlike metaphysical questions, are theoretically soluble." "Science, unlike metaphysics, does not always lead into deeper doubts . . . that is the cause of its progress in contrast with the stagnation of metaphysics. . . . Were there any room for doubt or scepticism, it could not build, for the superstructure would crumble down as rapidly as it was built up—even as we see in metaphysics." Thus I justify by Mr. Elliot's own declaration my position of agnosticism, which he treats with so much

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scorn. He says that metaphysical problems are insoluble : I agree. I have shown good reason for the assertion that the problem of the relation between brain and mind is a metaphysical problem. I see no justification, therefore, for Mr. Elliot's assertion that I carry scepticism to an unusually extreme degree ; and I find the less justification since the fact that the problem is a metaphysical problem is not my only ground for declaring it insoluble. I claim that I have proved by the strict canons of logic that it is insoluble. Mr. Elliot does not attempt to invalidate this proof. At present, therefore, my declaration that the problem is insoluble holds the field, for these concurrent arguments are opposed by nothing but Mr. Elliot's assertion that " the problem is one which beyond question can and will be solved theoretically, even, if it has not, as I venture to believe, been solved already."

I have said that Mr. Elliot sometimes goes beyond the limits of fair controversy, and I now proceed to substantiate the charge. Mr. Elliot seems to suppose that by this I mean to accuse him of using unduly strong expressions, and that I am complaining that he hits too hard. I made no such accusation. As Mr. Elliot very truly says, I myself use strong expressions when the occasion seems to justify them. I have said lately that a pornographic doctrine which has a wide vogue at the present time is a gospel of the Yahoos, and I see no reason to modify the expression ; but strength of language is not necessarily unfair, and unfairness is certainly not strength.

My first complaint of unfairness is that Mr. Elliot, by an adroit aposiopesis and discursion, conveys the impression that I am in the same boat with Sir Oliver Lodge, and am open to the same criticisms. Sir Oliver Lodge is an eminent electrician, and if my name and his were bracketed together as those of authorities of equal weight in a matter of electrics, I have no doubt that he would feel insulted, and he would be justified. On the other hand, when Sir Oliver Lodge's name and mine are bracketed together as those of authorities equally competent in metaphysics, I am not complimented. This, however, is a minor matter, and I pass on to one more serious.

In his attack upon Dr. McDougall, Mr. Elliot said, " No direct evidence of any kind has ever been found for the existence of a vital

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force." The context showed that by vital force he here meant the modification by the will of the action of the brain, and so of human conduct. To this I replied that "every exertion of the will that is followed by a bodily act or movement is evidence for the existence of a vital force," that is, of the effect of the will upon the action of the brain. I went on to say that though the sequence is not indisputable proof, yet it is evidence, and to show that it is generally received at least as evidence, and by most people as actual proof, I went on to say, "To the unsophisticated mind nothing appears more certain than that the mental operation of the will is the cause of the material movement," and then, by way of complement and contrast, "To the mind of the trained psychologist it is known that this very sequence is the foundation of our notion of cause and effect, whether it is in fact cause and effect or no." My meaning and intention are, I submit, perfectly clear. First, I adduce my evidence. How Mr. Elliot deals with it I will examine presently. Next, I assert that my evidence is accepted as proof by the unsophisticated mind, by which the context shows that I mean the minds of those untrained in psychology; and, further, is so relied upon by those who have been trained in psychology that they could not deny that it is evidence. The last part of this statement is altogether omitted by Mr. Elliot. Nothing could be clearer than my meaning. Human minds are for the purpose of my argument divisible into two classes—the unsophisticated mind that has not had a psychological training, and the mind of the trained psychologist. Perhaps I ought to have made a third class of the mind of Mr. Elliot. He seizes upon the expression "the unsophisticated mind," pretending that it includes only the minds of rustics and ploughmen, and pursues me through page after page with flouts and jibes and jeers at my sole reliance on the convictions of such minds, and he omits all reference to my reliance on the other class of minds also. This, I say, is unfairness.

My evidence is not the unsophisticated mind of the rustic and the ploughman. My evidence is the hard incontestable fact that a large proportion of our acts—Mr. Elliot's as well as the rustic's and the ploughman's and mine—are preceded by the exertion of will to do those very acts. I do not say that this sequence in time is proof of causation of the acts by the will. I do assert that it

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is *primâ facie* evidence. How does Mr. Elliot deal with this evidence? I say there is evidence, and I adduce my evidence. The only logical answer to this is to show that what I say is evidence is not evidence. What are Mr. Elliot's answers? They are :—

(1) Nothing *is* more certain than that a spiritual will does not by itself cause material movement." I said nothing about a spiritual will, but passing that, Mr. Elliot's reply is that delightful feminine argument, "Kindly allow me to know best!" It does not touch the question whether my evidence is or is not evidence. It merely begs the question. So does Mr. Elliot's unsophisticated rustic ploughman assert that there is no evidence that the earth is round. "But," says Captain Cook, "I have sailed round it." "That," replies the rustic, "is no evidence, for nothing *is* more certain than that the earth is not round."

(2) Mr. Elliot's second answer is that "There is not a single physiologist living who imagines . . . that muscular action follows immediately upon the operation of a spiritual will." *Ignoratio Elenchi*. What the soldier said is not evidence, nor is what the physiologist imagines. Still less is it evidence to adduce what the soldier did not say, or what the single (or even the married) physiologist does not imagine. In my evidence nothing is said about muscular action following immediately upon the operation of the will, nor is anything said about a spiritual will. There is nothing, therefore, in what the single physiologist does not imagine that traverses my evidence; still less is there anything that tends to show that my evidence is not evidence.

(3) "In his endeavour to controvert that statement [that no direct evidence of any kind has ever been found, etc.] . . . Dr. Mercier drags in the unsophisticated mind, which is known and admitted by all sides to be wrong." How foolish of me to drag in evidence that I knew and admitted to be wrong! Perhaps Mr. Elliot will kindly furnish me with the reference to my admission. He unaccountably omits to say that I drag in the sophisticated mind of the trained physiologist also. How came he to omit this?

(4) Mr. Elliot "cannot admit in favour of Vitalism the admittedly false superstitions of uncultured people." No one who knows the severely judicial cast of Mr. Elliot's mind could possibly expect him to admit as evidence admittedly false superstitions. It would

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be insulting to ask him to do so, and I have no wish to insult him. On the contrary, I desire to compliment him upon his ingenuity and cleverness, especially in avoiding and in begging the question at issue.

(5) Once more, what I adduce as evidence, as far as it goes, and for what it is worth, that mind acts upon brain, is the universal experience of mankind that a very large proportion of our acts are preceded by an exertion of will to do these very acts. This, I say, is evidence as far as it goes ; but if I should say that it is conclusive proof, I could adduce Mr. Elliot's authority for the statement, for what does he say on a subsequent page ?—

“ We human beings cannot go beyond what we see and experience. Our experience teaches us that this is an orderly universe : and as we increase our knowledge we find a certain number of laws to which no exceptions have ever been observed. One of these laws, and one of the most striking manifestations of the orderliness of our universe, is the law ” that certain of our acts are invariably preceded by an effort of will. “ It may not have an infinite and absolute validity, but its validity at all events is as high as any that can be attained by human knowledge. And that is all I care about. It is all, I think, that anyone need care about : certainly all that anyone can ever know.”

That is my position. As far as our experience goes, it is a law to which no exceptions have ever been observed that an effort of will to do those very acts is an invariable and necessary antecedent to some of our acts ; and that it seems to us from internal evidence (the only evidence we have) that we can act in this way or that, or refuse to act, merely by exerting our wills so to act or not to act. But to an invariable and necessary antecedent we give the name of cause.

As Mr. Elliot is unable to show that this is not evidence, we must take it that it is evidence, but evidence is not necessarily proof : it may be rebutted. Mr. Elliot does not even attempt to rebut my evidence, but there is evidence against it, and as he does not adduce it, I will adduce it myself.

(a) The first objection is that though we can form the verbal proposition that an exertion of will can modify the action of our brains, and so of our bodies, yet behind this verbal proposition there is no mental concept, and therefore, strictly speaking, it amounts to nothing. It is as though we said bitterness shrinks iron. We

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cannot imagine or picture to ourselves the action that we express by the words. To this the interacting dualist—the Vitalist, as Mr. Elliot calls him—would answer, if he knows his business, “It is quite true that I cannot picture in my mind the action, but we are here in the presence of one of the ultimate actions in Nature, and every one of them is equally unimaginable and impicturable. It is just as impossible to imagine how bodies at a distance attract one another through intervening space, but it is equally impossible to believe that they do not attract each other. It is just as impossible to imagine or picture to oneself by what means two electric currents in certain circumstances repel one another, but we have irrefragable evidence that they do. If we dive down to the bottom of the matter, it is just as impossible to picture to ourselves the ultimate mechanism by which, if we push one end of a rod in the direction of its length, the other end moves to an equal extent. In these ultimate experiences we are obliged to accept the evidence of experience in spite of our inability to imagine the method of operation.”

(b) “But,” the mechanist may further object, “the hypothesis of interacting dualism appears to require, for each occasion of interaction, a disappearance of energy into nothing or an appearance of energy out of nothing, and this, according to my notion of the constitution of the universe, is impossible.” This is how the mechanist would put his argument if he were a reasonable mechanist, but this is not how Mr. Elliot puts it. Mr. Elliot speaks in downright Dunstable language, and says, “It is proved that no creation of energy or of matter ever takes place.”

“Is it proved?” I answer “then show me the proof.”

“Oh! you have no business to ask for proof of such a thing. I take it as represented by the entire body of physicists. It is the thickest pillar of physics and chemistry. It is one of the most certain items of human knowledge.”

“This may be so,” I answer “but you offered me proof. Where is it?”

“I agree that no evidence can be offered to prove that it always and incontestably has held true.”

“Then why offer proof that you cannot produce?”

“Well,” Mr. Elliot, if he were candid, would answer when driven

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into this corner, "I was attacking Vitalism, and any stick is good enough to beat Vitalism with. Besides, though I am fond of exposing random statements made by others, I admit with reluctance that I am only human, and sometimes make them myself; and anyhow, I thought I was safe, and did not expect my assertion to be critically examined, but I will be more careful in future."

"That," I should reply "is all I desire. Let us shake hands upon it."

But in my temporary part of a Vitalist I have to meet not only Mr. Elliot's exaggerated claim that it is proved, etc., but the moderate statement of the reasonable mechanist that I have given above. It does seem that interacting dualism requires the disappearance and reappearance of energy, and I am with you that in the material world it is extremely unlikely that any such disappearance and reappearance take place; but, in the first place, I would have you remember that so many things that seem *prima facie* unlikely, and even impossible, have been found to be true. It seems incredible and impossible that not-living matter should be transformed into living matter, and we cannot understand how the change takes place, but we witness the transformation every day. It seems incredible that unconscious matter should become conscious, and we do not understand how the change can take place, but we witness the change every time a human being comes into existence. It seemed incredible only a few years ago that any substance that we knew as an element should disintegrate itself with violence and continuously. The elementary nature of the substances we called elements was accepted by the entire body not only of chemists, but of physicists as well. It was the thickest pillar—well, then, the second thickest pillar—of physics and chemistry. The whole of chemical science rested on it. It was one of the most certain items of human knowledge, and now what is become of it?

And after all, Vitalism, as you call it, does not really need the annihilation or creation of energy. An alternative hypothesis is that energy may be transformed into mind, and mind transformed into energy. Thus the disappearance and reappearance would balance, and there would be neither creation nor annihilation. I am quite familiar with your objection that the change is inconceivable, and that you cannot imagine how it could happen, but then

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so many things happen without your being able to conceive how they happen that I am not much moved by this argument. It is, no doubt, a more serious objection that you have been familiar with the hypothesis since you were seventeen, but even this is not, to my mind, conclusive. You say that no item of evidence has ever been found for it. I beg your pardon. You are mistaken. Every production of a sensation in the mind by the impact of energy upon the body, every sensation of touch, hearing, sight, temperature, and so forth, is evidence that energy can be transmuted into mind ; and every exertion of will that is followed by bodily action is evidence that mind can be transmuted into energy. I do not say that these experiences are proofs of the transmutation, but they are unquestionably evidence. If you say you cannot understand how the transmutation can take place, I am with you. There are many things that I cannot understand. There may be some that even you cannot understand, and possibly this is one of them.

You tell me that " the general structure of the nervous system is such as to offer the most insuperable difficulties in the way of such an hypothesis." Is it indeed ? What are these insuperable difficulties ? Do you know what structure favours and what structure opposes the transmutation of energy into mind, and of mind into energy ? Is not the transmutation, if indeed it takes place at all, an operation, on your own showing, unique in Nature ? and is not the structure of the nervous system unique in Nature ?

(c) A third objection to my evidence in favour of interacting dualism might be made by Mr. Elliot, and as he does not make it, I will present him with it. It is true, he might say, that an exertion of the will to do that very act does precede each voluntary act, but it does not follow that the sequence is a sequence of cause and effect. It may be merely a temporal sequence of antecedence and subsequence. If I were a " Vitalist " I should answer by asking how a causal antecedent is distinguished from an antecedent that is merely temporal. What say the authorities ? Mill says a cause is a necessary antecedent, an antecedent without which the event would not have happened. Well, the exertion of the will answers the description. If a certain action within our powers is suggested to us, or occurs to us, this action is not undertaken until and unless we exert our volition and will to do it. As long as we

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suspend our volition it is not done. Unless we exert our volition it is never done. The instant we will it, the action proceeds. This is what convinces the unsophisticated mind, for which Mr. Elliot has such a profound contempt, that will is the cause of action. But I do not appeal to the unsophisticated mind alone. I anticipated Mr. Elliot's objection to unsophisticated minds, and in the passage that he suppressed I carried my appeal across to the sophisticated mind of the trained psychologist, and said, what no trained psychologist will, I think, deny, that the sequence of voluntary action on the exertion of the will is the sequence at the foundation of our whole concept of causation. It is the very earliest instance of what seems to be causation that we experience. "Among primitive peoples," says Mr. Elliot, "the occurrence of an event for which no mechanical cause can be assigned is attributed to an animate cause." Why, of course it is. They must: they cannot help attributing it to the only known cause which, in their experience, accounts for events—their own voluntary movements—for which no mechanical cause can be assigned. Nor can we; and this is why the residue of events, for which no mechanical cause is known, is always ascribed to animate causes. This residue is now become very small. But the fact that animate causation, or causation by volition, has been illegitimately ascribed to events to which such causation is not applicable does by no means prove that volition is never a cause of mechanical movement. It was for many centuries thought that the relative positions of the planets with respect to one another were the cause of epidemics of disease among men; this opinion is now believed to be erroneous, and epidemics are ascribed to other causes, but it does not follow that the relative positions of the planets leave no causal influence at all. They may still affect the movements of the planets. It is by assimilating other instances of antecedence and subsequence to the sequence of bodily action upon volition that we build up our general concept of causation. If we had never, by the exertion of our wills, executed bodily acts, we should have no concept of cause and effect; and if we could completely divest ourselves of the belief that will is the cause of action we should possibly at the same time divest ourselves of the concept of causation.

These are the answers I should give to Mr. Elliot if I were what

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he calls a Vitalist ; but I am not, and I am not so much concerned to show that " Vitalism " is true, as to show that at least as much can be said against Mechanism, if by Mechanism Mr. Elliot means psycho-physical parallelism, as against Vitalism. Psycho-physical parallelism has many variants. It would take too long to examine them all. I confine myself to two, the earliest and one of the latest.

The earliest psycho-physical parallel was the Leibnitzian doctrine of pre-established harmony, of the two clocks, cerebral and mental, set going at birth at the same rate, to preserve simultaneity throughout the rest of life, so that when a certain process takes place in the brain, a corresponding process takes place in the mind. There is no direct connection whatever between them, but yet they always happen together, and the correspondence is not only in time, but in degree, in quality, and in other respects. The mental and cerebral clocks are so regulated at birth than when, fifty years afterwards, the aerial commotion caused by a pistol-shot produces, through the auditory nerve, a commotion in the brain-clock, it happens that the mind-clock strikes one, and we hear the sound. If there are people who can believe this, I envy them their credulity ; but even they are obstinate sceptics compared with those who can believe that when the brain-clock is accidentally damaged, the mind-clock is so set that, without being in the least affected by the damage to the brain, it yet suffers damage simultaneously.

The doctrine of epiphenomenalism—blessed word—while less intrinsically absurd, is open to equally damaging objections. According to this modification of the mechanist doctrine, as long as the current of nervous energy flows unobstructed in its channels, it has no mental accompaniment, but when it reaches a narrow channel, through which it is forced with difficulty and with friction, it is accompanied by the epiphenomenon of mind, just as the passage of electricity, when it passes with friction through the tenuous filament, is accompanied by the epiphenomenon of light. I believe as far as this the facts are as stated, and that it is under these circumstances that sensations, and perhaps certain other states of mind, arise. But although analogies and parallels must not be pushed too far, it is to be remembered that the nerve current, like the electric current, is a physical event ; and we cannot ignore the fact that in

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the production of light some of the electricity is used up and disappears, being in fact changed into light ; and by the very doctrine of conservation of energy to which the mechanists pin their faith, the same thing must happen with the nerve current, or an effect is produced without a cause, which the mechanist should be the last to admit. And, apart from this, the hypothesis accounts only for the ingoing current and its effects or accompaniments. It ignores altogether, as Huxley ignored, the case of the will. In this case the evidence of our own minds, which is all the evidence we have, goes to show that the mental event is not epiphenomenal, but precedes the bodily event. As far as it is possible to ascertain from introspection, which is our only source of information, it appears that volition is an original and initiating process, guided, indeed, as to its direction, by brain structure, but owing its origin to the mental self alone. Mr. Elliot says that Huxley would have disagreed with me, and I admit that it is possible he would ; but Huxley never faced the problem of volition, and Huxley never had the advantage of having my arguments put before him ; and, moreover, I am not sure that Mr. Elliot is now in Huxley's confidence.

If volition were indeed a mere epiphenomenon of nervous action, then we should be automata—conscious automata, but mere automata—and human volition would be a vast imposture. Determinism would be the only law, and responsibility would be gross cruelty. This may be so, but Mr. Elliot cannot admit it without utter inconsistency, for he is an ultra-empiric, and it is incontestable that the whole conduct of the whole human race towards one another has always been founded on the supposition that the will is free ; and—this is the conclusive test—conduct founded on this belief has never brought us up against experience that contradicts it. This, as I have argued in my book on Psychology, is the ultimate and unpugnable test of truth. Whether the hypothesis is true or not in some absolute sense is beyond our ken and beyond our means of investigation. Its non-contradiction in universal experience constitutes the test, the only test, the sufficient test, of its truth for us. We are precluded by the constitution of our minds from doubting it. We may pretend to deny it : we may verbally deny it, just as we may pretend to doubt the existence of a world outside

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our own minds, and verbally deny the existence of matter ; but when we are put to the test and are obliged to act, we prove by our action that we conclusively believe, that we are precluded from doubting, that which we have professed to doubt.

One or two minor contentions of Mr. Elliot must be refuted, lest he should assume that they cannot be refuted. He is contemptuous because I "bolster up my theory with a very heterodox view of the facts." This is because I assert, and claim to have shown, that the action of the nervous system is not wholly reflex, but is to a very important extent autogenic ; and that I take this heterodox view is, Mr. Elliot says, a strong presumption against my theory. I should not be inclined to yield to this criticism even if I were trying to bolster up a theory ; but, as Mr. Elliot admits with some indignation, I advance no theory, and hold no theory, of the connection of brain and mind. But if I had, I distinctly said, and now repeat, that whether the action or structure of the nervous system is reflex or autogenic has no bearing whatever upon any theory of the connection of brain and mind. My objections to his adducing the reflex structure of the nervous system as an argument in favour of mechanism were two. First, that, whatever the structure of the nervous system, its action is certainly not wholly reflex ; and, second, that whether the structure or the action is reflex or not has nothing to do with the question of Vitalism *versus* Mechanism.

The same objection, that I have to bolster up my theory with a very heterodox view of the facts, applies, Mr. Elliot says, with even greater force to my requiring him to substantiate his statement "it is proved that no creation of matter or energy ever takes place." How I can bolster up a theory when I have no theory to bolster, I do not know, and still less do I know how I can bolster up my non-existent theory by asking Mr. Elliot to produce a proof of his. He says there is a proof, and when I ask for it, he says the proof is that many people—the entire body of physicists—believe it. But then, many people, many more than the entire body of physicists, believe that the will causes bodily movements. If I must adhere to the one belief because many people adopt it, why may I not adhere to the other because more people adopt it ? What is sauce for Mr. Elliot's goose is surely sauce for Dr. McDougall's gander.

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Many more people have believed for a much longer time the doctrines of judicial astrology than have believed in the non-creation of matter and energy. Ought I, then, to believe the doctrines of judicial astrology? It is true that the believers in judicial astrology were not the entire body of physicists, but they were the entire body of astrologers, and surely they ought to know as much about astrology as the physicists know about physics.

As for the grotesque instances which Mr. Elliot adduces of whales dancing on their tails, and so forth, I should not like to be called upon for proof that they do not exist, but then I should be careful not to assert that it had been proved that they do not exist; and if I were so incautious as to make such an assertion, I should not make it a grievance if I were called upon to substantiate it.

If the case of *Elliot v. McDougall* were to be tried in a court of law, I could not go into the witness box and swear that in my opinion the doctrine of either is true. If Mr. Elliot prosecutes Dr. McDougall for the nuisance of promulgating a false doctrine, and I am on the jury, I must find a verdict of not guilty; not because I hold Dr. McDougall's doctrine to be true, but because the prosecutor has to prove his case, and unless he can convince me beyond reasonable doubt that Dr. McDougall's doctrine is false, I am bound by my oath to acquit. And the prosecutor has not proved his case. I was in doubt before, and I am still in doubt. On the other hand, if it is a civil action, and I have to return a verdict representing, not a conviction of certainty, but the balance of probability left in my mind by the evidence, the verdict is still for the defendant.

I now transfer myself from the jury box to the bench, and I thus sum up the case to the jury:—

Gentlemen of the Jury, the issue you have to try is a very simple one. The plaintiff alleges that the doctrine taught by the defendant is false, and that various persons have suffered damage thereby. The doctrine that is alleged to be false is that the exertion of the will is the cause of the voluntary acts of our bodies. You have heard the witnesses, twelve in number, who have testified on behalf of the plaintiff, only four of whom have been cross-examined. You may, therefore, accept the evidence of the other eight without hesitation; and with respect to the four who have been cross-

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examined, it is for you to say whether they have emerged from this ordeal without damage to their credit. It is not enough, however, for you to be satisfied that the plaintiff's witnesses are speaking the truth. You must be satisfied also that their evidence contradicts the defendant's doctrine. If you are satisfied on both points, you must return a verdict for the plaintiff ; but if you find that although the witnesses are witnesses of truth, yet their evidence, true though it is, does not contradict the defendant's doctrine, you must find for the defendant. In other words, you must put to yourselves, with respect to the evidence of each witness, two questions,—first, Is it true ? and, second, Is it relevant ? Unless you can answer both these questions in the affirmative with respect to the evidence of one of these witnesses, you must find for the defendant ; but if you find that the evidence of even one witness is both true and contradictory of the defendant's doctrine, you must find for the plaintiff, with such damages as seem to you just and proper. The evidence given on behalf of the plaintiff is as follows :—

(1) “ Among primitive peoples the occurrence of an event for which no mechanical cause can be assigned is attributed to an animate cause.” I must explain to you, gentlemen, that by an animate cause the witness means the exertion of a will. Does this evidence contradict the defendant's doctrine that the will causes bodily movements ? The witness does not say that the will does not cause bodily movements. He says that certain people believe that will does cause events, by which he means presumably events other than bodily movements. It must be clear to you that this belief on the part of primitive people, whether the belief is true or false, does not contradict the doctrine of the defendant.

(2) “ Among all peoples, animate causes are invoked for the explanation of all events that are not known to be mechanical in Nature.” This belief is not couched in the same language as the previous belief, but we must presume that it is intended to mean the same thing. According to this witness, more people hold this belief than were known by the previous witness to hold it. You will probably agree that this wider prevalence of the belief adds something to the probability that it is true ; but still, if it is true, the fact that the will does cause some events is no contradiction

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of the doctrine that it causes others. On the contrary, it is to some slight extent a corroboration of the doctrine.

(3) "As science has, step by step, brought one class of facts after another from darkness into light, the animate cause has in no single instance been found correct. In every case the cause has been found mechanical." This witness, you see, discredits the belief testified to by the previous witnesses. That belief was to some slight extent a corroboration of the defendant's doctrine. This slight corroboration is now swept away, and the matter stands precisely where it was before any of these witnesses gave evidence.

(4) "At the present day, animate causes" (I must repeat that by animate causes the witness probably means the will of some being or beings) "are assigned (as in the past) only in those spheres where knowledge is still vague." Clearly this evidence does not contradict the doctrine that the will causes bodily movements. It shows that where knowledge is vague, and with respect to the causation of bodily movements our knowledge is vague, the will is assigned as a cause.

(5) "As in the past, the sole evidence offered for them" (this means for the causation of bodily and, perhaps, other movements by the will) "is that mechanical explanations have not yet been proved."

(6) "No direct evidence of any kind has ever been found for the existence of a vital force. It is unknown to physicists and chemists." I must explain that by a vital force the witness means the action of the will upon the brain. You will think it strange that the plaintiff's witnesses should thus vary their terminology without rhyme or reason, but we must take them as we find them. These two witnesses are directly contradicted by the defendant, who brings forward the evidence whose existence they deny. This counter-evidence is not rebutted by the plaintiff, who does not deny that it is evidence, nor dispute its value. On this point, therefore, you must give credence to the defendant.

(7) "Reflex action is proved to be mechanical." Perhaps some of you gentlemen can see the relevance of this evidence. For my part, I cannot see what bearing it has on the question whether will can cause bodily movement.

(8) "The whole nervous system is built up on the reflex principle, and forcibly suggests mechanism." This witness is playing on the

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word "mechanism." The plaintiff gives to his denial that the will can cause movements the name of mechanism. This name is given also to an arrangement of material parts into a whole for the redistribution of motion. No one questions that the nervous system is in the latter sense a mechanism, and the plaintiff urges that this is an admission that it is mechanism in the former sense. This is what logicians call equivocation.

(9) "Until reflex action was proved to be mechanical it was alleged to be vitalistic." Here the witness is similarly playing upon the word "vitalistic." This is another of the many names the plaintiff gives to the causation of bodily movements by the will. It is also a name for all those processes that take place in living bodies alone. Reflex action is such a process, and is therefore vitalistic in the second sense. The plaintiff asserts that as it may have been called vitalistic, it was called vitalistic in his peculiar sense of the word. As no one pretends that the causation of movement by the will is reflex action, you will see that the evidence of this witness is not relevant to the question at issue.

You must now take into consideration what the plaintiff appears to be groping after in adducing all this evidence, each item of which, taken by itself, is totally irrelevant when it is not equivocation. He does not put it plainly, in fact he does not put it at all, but he seems to be hinting at some such argument as this: The will was formerly thought to be the cause of many events which are now shown to be caused otherwise. It is still thought to be the cause of bodily movements, therefore in time it will be shown that it is not the cause of these movements. Now, gentlemen, if this is really what the plaintiff is hinting at, you will see that he admits that up to the present time it has not been shown that the defendant's doctrine is false. All he says is that there is some evidence that in course of time it will be found to be false. What is the value of this evidence? May we safely infer, when a certain thing has been wrongly attributed as a cause to certain events, that it cannot be the cause of certain other events? Let me give you an historical parallel. A hundred years ago it was conclusively presumed that Napoleon Bonaparte was the cause, not only of the invasion of Russia, but of the rise in the price of corn, of the rising of the Lud-dites, and of the invasion of butchers' shops with large blue flies.

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This mode of causation has been entirely abandoned with respect to the last three events, but research has confirmed our belief that it was correctly assigned in the first case.

(10) "Vitalism involves a creation of energy or of matter. It is proved that neither takes place." The evidence of this witness is completely broken down in cross-examination, and the plaintiff himself abandons it. It would be most unsafe to place any reliance upon it.

(11) "Belief in vitalism in any society is proportional to the ignorance prevailing in that society; belief in mechanism is almost solely confined to *savants*, and among them is most widespread with physiologists whose knowledge of the facts is greatest." It is impossible to say in what senses this witness is using the terms "vitalism" and "mechanism," but supposing that vitalism means the production of bodily movements by the will, and mechanism means the non-production of such movements by the will, the fact, if it is a fact, that certain persons believe in mechanism is, by the plaintiff's own showing, no proof that mechanism is true, nor is it any contradiction of vitalism.

The twelfth witness is merely abusive. He offers no evidence, and you must disregard what he says.

This analysis of the evidence of the plaintiff's witnesses does not disclose one particle of evidence in contradiction of the defendant's doctrine. This doctrine may be true or false: you may have your own opinion about that; but in this court you are to determine the issue by the evidence placed before you. You are not bound to find the defendant's doctrine true. You may suspend your judgment. The plaintiff declares that it is false, and it is for him to make out his case, and prove that it is false. He has had a great deal of latitude, and has been permitted to introduce a quantity of evidence about reflex action, and so forth, that is totally irrelevant. If Dr. McDougall had been well advised, and had submitted that there was no evidence to go to the jury, I could have stopped the case on this ground. As it is, it is for you to say whether the plaintiff has succeeded in producing any evidence at all in contradiction of the defendant's doctrine.

The Clerk of Arraigs: Gentlemen of the Jury, do you find for the plaintiff or for the defendant?

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The Foreman of the Jury : We find them both Guilty, but we recommend Dr. McDougall to mercy because he meant no harm.

The Judge : Mr. Hugh Elliot, you have been found by a jury of your countrymen guilty of a determined, premeditated, and unprovoked assault upon an innocent doctrine that, whatever its defects, has never done you any harm. The fact that you have not inflicted the slightest injury upon it does not exonerate you from blame. You did your worst, and if your worst amounts to nothing, that does not mitigate your guilt. I have no desire to be hard upon you, and the sentence I am about to pronounce is intended not as a punishment, but purely and solely to reform you and to enable you to lead a new life. That it must be exquisitely painful to a person of your temperament is a matter which I regret, but justice must be done, and the punishment that you must suffer is the least that justice requires. The sentence of the court upon you is that you be imprisoned in a poultry farm, in which the early village cock's shrill clarion each morn will rouse you from your lowly bed. There you will study *Best on Evidence* and *Schiller on Relevancy* until you can conduct an argument in a convincing manner. Remove the prisoner.

As for you, Dr. McDougall, your case is a sad one. You have displayed cowardice in the face of the enemy. Instead of meeting the arguments of your opponent, you have sheltered yourself behind Merz and Hobhouse, and actually confess that you are at a loss what to say. When your adversary becomes flippant, you become abusive. This is no way to conduct a scientific, or rather, a metaphysical controversy. More than this, it is painful to find that you are the associate of suspected persons. You have been consorting with the Society for Psychical Research. It is sad to see a man of your antecedents, your learning, your powers of reasoning, and, let me add, your nationality, fallen so low. I am bound, however, to take into consideration the recommendation of the jury, and shall take upon myself to liberate you under the First Offenders Act, but on this condition, which must be punctually observed—that in future everything you write must be written with a pencil hermetically sealed up in a glass bottle.

(Loud applause in court, which was immediately suppressed ; and a voice, " Votes for Women ! ")

DIRECTIONS OF RECENT WORK ON THE INHERITANCE OF “ACQUIRED CHARACTERS”

By H. M. Fuchs

IF it be accepted that animals and plants as they exist to-day are descended from ancestors different from themselves, some theory must be formed to account for the changes. Lamarck put forward the idea that as organisms adapt themselves during their lifetime to a gradually changing environment, their offspring are born to a certain extent already possessed of the adaptations acquired by their parents. Darwin thought that the chief means by which succeeding generations changed with the environment was the survival in the struggle for existence of those *chance* variations which happened to give the greatest advantages to their possessors. He accepted Lamarck's view, however, as affecting the course of events to a certain extent, that is to say, the variation seen among the members of a given family was not quite random, but was partly influenced by the adaptive changes undergone by the parents.

As it is very obvious that living organisms adapt themselves to environmental changes, the supposition that permanent adaptations have been produced by the inheritance of such reactions is very natural. Quite a new aspect was, however, put on the matter by the teaching of Weismann regarding the relation of the body to the germ-cells it bears. Before his time it was supposed that the fertilised egg developed into an individual, which, when it became adult, produced eggs or sperm as the case might be. The egg then united with the spermatozoon of another individual, and so the cycle was repeated. Weismann, as is well known, looked at the matter in another light. He said that the fertilised egg gave rise

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at the same time to the body (soma) and to the future eggs or spermatozoa (germ-cells). Thus the body did not *produce* the latter, but merely *carried* them. The germ-tissue persisted through the generations, at each of which a body was developed as a carrier for it. This point of view, in which the germ-cells are something apart from, although inside, the body, profoundly modified biological thought, and it came to be questioned more and more whether changes undergone by the soma could affect the hereditary qualities of the germ-cells at all. Inheritable variations exhibited by the offspring might be altogether due to internal causes within the germinal tissue.

Now it has been recognised for some time past that the description of this problem as that of the "inheritance of acquired characters" is a bad one; for apart from the special use of "acquired," meaning a modification acquired *by the soma* in response to external stimuli, the term includes at least two distinct possibilities. In the first place, a certain external stimulus might produce a change in the soma which would then so affect the germ-cells that the next generation would develop the same altered character without the reapplication of the stimulus. In the second place, the stimulus might affect both the soma and the germ-cells *at the same time*, producing a change in the former and causing the latter to transmit a similar change to the succeeding generations. The first possibility is the "inheritance of acquired characters" *sensu stricto*, but is better described as Somatic Induction. This serves to distinguish it from the second possibility, or Parallel Induction.

It seems to be well established now that external causes can alter the hereditary qualities of the germ-cells by parallel induction. Probably the best known instances are the experiments of Standfuss* and Fischer.† Pupæ of butterflies were subjected to abnormally low temperatures, by which means variations in pigmentation were induced in the adults which emerged. The offspring of these changed individuals, reared under normal conditions, showed to greater or lesser extents the aberrational markings of their parents. The presumption is that changes were induced in the germ-cells

* *Insektenbörse*, 16, 1899.

† *Allg. Zeitsch. f. Entomol.*, 1901.

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by the low temperatures, in the same direction and at the same time as those induced in the somatic cells.

It should be mentioned as well that there is another possibility with regard to the direct influence of external conditions on the germ-cells. The latter may be affected with respect to their hereditary qualities without a corresponding change being produced in the soma at all—that is to say, the induced character appears for the first time in the offspring of the animals or plants experimented on. Such a case cannot of course be far separated physiologically from parallel induction, in which both the soma and the germ-cells are altered in the parental generation; nevertheless it would not be classed as the “inheritance of an acquired character.” W. L. Tower’s * extensive experiments with the potato-beetle (*Leptinotarsa*) in America showed that when animals were kept in unusual conditions of temperature and moisture, inheritable colour-pattern changes were induced which appeared for the first time in the offspring, the parental soma being unaffected.

It is, however, with somatic induction—the influencing of the hereditary qualities of the germ-cells by changes induced in the soma—that we are particularly concerned here. Now, although the possibility of somatic induction remains as hotly debated a subject to-day as it has been in the past, there is a great difference in the present-day method of attacking the problem. Formerly, in any discussion on this topic, lists of cases were adduced which “could not be explained otherwise”—no other theory as to how permanent changes in the characters of species were brought about would be able to account for them. Of recent instances of such argument two cases may be quoted. Semon † confirmed Darwin’s observation that in human embryos the skin of the foot-sole is already considerably thicker before birth than is that of the rest of the body. This thickening must primarily have been an effect of use, and thus, according to Semon, we have an example of the inheritance of a character originally developed in the soma in response to external conditions. Zederbauer ‡ gives the following case which concerns the shepherd’s purse (*Capsella*). There is a variety

* *Heredity and Eugenics*, 1912.

† *Das Problem der Vererbung erworbener Eigenschaften*.

‡ *Oestr. bot. Zeitsch.*, 1908.

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of the plant having short hairy stems and reddish flowers which grows in Asia Minor at a height of 2,000 to 2,400 metres. It was probably introduced into the uplands along a road which is two thousand years old. The lowland *Capsella* has a high stem and white flowers. When seeds of the latter were caused to germinate in high altitudes, plants were developed resembling the highland type. This seemed to indicate that the features of the natural highland type were a result of direct somatic response to changed conditions. If this be so, the characters have certainly become fixed and inheritable, for seeds of the natural highland form, when planted at low altitudes, gave plants with the stem and floral characters unchanged, a condition which persisted through a number of generations. It is not clear, however, that the highland form did not originally develop from the lowland by the natural selection of individuals which varied in a direction happening to suit the environment. On the other hand, if the highland characters were developed as a direct result of new conditions, the latter might have affected the germ-cells (and thus the next generation) at the same time as they influenced the soma.

The citation of such circumstantial evidence, however, cannot give an unequivocal answer to our question, any more than can the elaborate logical analyses *pro* and *con.* of Spencer and Weismann: for in all such cases some of the incident conditions, which it is claimed have influenced the soma, and through the latter the germ-cells, have been in past time. We cannot be perfectly certain that a change in the characters of a race can be brought about in this way unless all the factors are at work under our eyes and are thus controllable. In other words, the problem is one which must be attacked by direct experimentation, a position which has been recognised for some time past by investigators of heredity.

As soon as the matter is looked into, it is seen that the theoretical difficulties attending such experiments are great, since one must be able to be perfectly certain that any inheritable effect obtained is due to no other cause than somatic induction. Modern genetic research has taught us the necessity of working with pure, or homozygous, races, so that we can be certain that there is no possibility of the experimental animals or plants throwing recessive offspring, the characters of which might wrongly be put down to external

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influences having affected the parents. There is no difficulty, of course, in selecting for the experiments animals of which the genetic history has been thoroughly worked out by preliminary breeding. It is in another direction that we meet our main difficulty. How are we to be certain that the changed environment to which we subject the organisms does not *directly* affect the germ-cells at the same time as it modifies the soma? If this possibility be not excluded, any inheritable differences appearing in the characters of the offspring might have been produced by parallel, not somatic induction. Tower * has made an analysis of the conditions under which experiments must be made if they are to give an unassailable result. In order to obviate the possibility of any direct effect on the germ-cells it must be known beforehand at what stages (if any) in their development the germ-cells can be influenced by the incident forces used to effect the somatic modifications. When this is known, the somatic change must be induced at a time when the germ-cells have been found *not* to be sensitive.

The soundness of this precaution is obvious, but the carrying out of it is far from easy, as may be illustrated by an experiment made by Tower himself. Beetles of the genus *Leptinotarsa*, taken from races of known genetic constitution, were subjected to altered conditions of temperature and moisture, by which means their colour patterns were changed. The offspring of these insects were reared under normal temperature and moisture conditions, to see if any alteration in pigmentation had been transmitted to them. Incidentally it was found that their characters were uninfluenced, but the point that particularly interests us here is how Tower carried out the necessary conditions of experiment laid down by himself. In order to prevent any direct influence on the germ-cells by the incident conditions used to influence the soma (in this case changed temperature and moisture), the beetles were replaced in normal surroundings during the period of growth and maturation of the germ-cells. Now although it may be possible to show that during this period the germ-cells are most easily influenced by external forces, yet the assumption is involved that they cannot be affected at all in the pre-growth stages.

* *Loc. cit.*

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This is the real central difficulty. Can we ever be certain that at a particular stage the germinal tissue is quite uninfluenceable by our changed environment? Greater precautions than those taken by Tower may not be humanly possible, when the only alternative would be that of taking the germ-cells out of the body, causing an alteration in the characters of the latter, and then putting the germ-cells back again! This is, of course, an impossibility, but it leads us on to the second line of attack which might solve the problem, namely the transplantation of the gonad from the body which produced it to that of a "foster-mother."

A female of a race bearing certain definite characters may be castrated, and into her body be transplanted the ovaries of a female coming from a race bearing other definite characters. This can and has been done in a number of cases in different animals, and it is found that in a certain proportion of operated individuals the new ovary continues to grow in the foster-soma, producing mature eggs. If these eggs be then fertilised by spermatozoa of a male of known genetic constitution (or better still, made to develop parthenogenetically), it can be tested by examining the individuals they give rise to whether they still transmit the characters of the stock from which they were taken, or whether their sojourn in the body of the foster-mother has caused them to transmit *her* characters.

In this type of experiment there is no induction of a new modification in a soma in order to see if the change will affect the hereditary qualities of the germ-cells. We merely test whether eggs which are forced to develop in a "foreign body" transmit the characters of their own race or those of the foster-mother. In the latter event it would clearly be shown that germ-cells can transmit characters impressed on them by the soma.

In transplantation experiments the objection that changed external conditions might directly affect the germ-cells is obviated, but other difficulties arise. There is the possibility of the regeneration of the original ovaries in the female used as foster-mother, but this can always be tested by a *post-mortem* examination. The new environment, again, in which the graft is placed might cause new activities in the latter, quite independently of any possible transmission of characters. There is, however, a way in which this can be controlled. An actual transmission from the foster-soma

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to the implanted graft should show *introduced* characters of the former in the offspring of the latter, while a change due merely to the new surroundings of the ovary should give only *altered* characteristics.

An account of recent experiments with transplanted ovaries is given below, but before dealing with them we will turn to the latest of an extensive series of investigations made by Paul Kammerer in Vienna.* The earlier experiments of this investigator with amphibians and reptiles are well enough known; the following summary refers only to his latest publication (1913) on the inheritance of artificially induced colour variations in the spotted salamander.

The coloration of this salamander consists of irregular patches of yellow and black pigments, the relative amounts of the two varying considerably in different individuals. For the first experiments Kammerer chose a number of young animals in which the black pigment predominated, and kept them on yellow loam-earth. He found that as the animals grew under these conditions the yellow pigment gradually increased at the expense of the black, the original yellow patches growing in size and new ones appearing in the black areas. The offspring of these animals were found, at the time of metamorphosis from the aquatic larva to the adult form, to be considerably more richly marked in yellow than their parents had been at a corresponding stage. The reciprocal experiment, in which relatively yellow individuals were grown on black garden earth, produced an increase in the amount of black pigment. The offspring of these showed at metamorphosis relatively more black than their parents had exhibited at the same stage.

Thus a character induced in the parents had made its appearance in the offspring without a reapplication of the external stimulus (here the colour and nature of the ground), for the larval offspring had been reared on neutral coloured sand up to metamorphosis. It is a great pity that none of the descendants were grown on a neutral ground *after* metamorphosis, in order to compare their pigmentation when fully grown with that of their parents. Instead of this each family was divided into two lots, one of which was grown on yellow

* *Arch. f. Ent.-mech.*, Bd. 36.

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ground and the other on black. In the case of the children of parents which had artificially been made yellow, those grown on yellow loam showed a still greater increase of yellow pigment, while in those on black earth the amount of yellow gradually decreased again. The case was similar with the families from artificially black parents.

From these first generation descendants a second generation was reared. Those animals of which both the grandparents had been grown from metamorphosis to maturity on yellow loam exhibited at metamorphosis a still greater amount of yellow pigment than their grandparents had shown at the same stage. Thus there seemed to be a cumulative effect of the external conditions when applied throughout several generations, but, as stated above, none of the altered offspring were reared and bred on *neutral* ground to test the permanency of the induced change.

One very remarkable effect was obtained. All offspring of animals which had been kept on yellow or on black grounds showed from metamorphosis onwards a bilaterally symmetrical arrangement of the pigmentation. Now there is in nature a variety of this salamander (*S. maculosa*) in which the colours are arranged in symmetrical longitudinal stripes, but Kammerer found that the spotted animals never produced striped young so long as they were kept on neutral ground. It follows that the experimental conditions had induced two inheritable effects, firstly the reappearance in the young of the artificially induced pigmentation of the parents, and secondly the appearance in the young of a character not exhibited by the parents, namely bilaterality of the markings.

Kammerer meets the criticism that his original animals may not have been pure-bred for the characters considered, and that the new markings of the offspring may, therefore, have been only due to the re-appearance of characters introduced into the back ancestry by crossing, with the statement that under normal conditions of humidity, temperature and colour surroundings there is little variation in the relative amounts of the pigments. Further, as mentioned above, the offspring of animals kept on neutral ground were all irregularly spotted like their parents. It is very unfortunate, however, that no figures are given in these main experiments of the numbers of animals used and the numbers of their offspring

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reared. Although the impression is left, after reading the paper, that considerable numbers of individuals were made use of for the experiments, yet the very important matter of the exact counts is omitted. Without this datum the difficulty of forming an opinion as to the real value of the results recorded is greatly increased.

Besides differing in colour, the substrata used in the experiments differed in the relative amounts of moisture they contained: the yellow loam was always damper than the black earth. By further experiments Kammerer isolated the colour and humidity conditions from one another, investigating their separate effects on the salamanders. For the colour factor alone, animals were grown on yellow and black *papers*, the humidities of which were equal. It appeared that the effects obtained were similar to those with the earths, but that the pigmentation changes occurred by increase and decrease of pre-existing patches alone, no new spots appearing. The offspring were bilaterally symmetrical as before. To eliminate the colour factor and test the effects of different humidities, the animals were kept on neutral coloured sand containing different amounts of water. Dampness caused an increase in yellow pigment, but this was brought about by the appearance of new spots in the black areas, the original yellow patches not altering in extent. In the same way dryness produced black spots in the yellow regions. The newly metamorphosed young showed markings similar to those induced in their parents, but always *without* the bilaterality.

These experiments showed that with the yellow loam-earth first used colour and dampness worked together in inducing an increase of yellow pigment in the salamanders, while with the black earth dryness and blackness both produced an increase in the black pigmentation. The bilaterality in the markings of the descendants was, however, due alone to the effect of the *colour* of the substratum on the parents.

The induced colour changes in the animals took place by an actual growth and proliferation of the pigment cells, not by movement of chromatophores, as is the case in fishes, octopus and many other animals. The next question was then, by what means does the nature of the ground produce this effect on the pigment-bearing cells? It was found that blinded animals showed none of the yellow and black ground reactions, so that the stimulus inducing

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the change must work on the eyes and through the nervous system. The humidity reactions, on the other hand, occurred in blinded salamanders exactly as in ones that could see. The effects of the relative amounts of moisture, then, were occasioned directly through the local sensibility of the skin. This point will be referred to below, when discussing whether the effects produced in this investigation were to be attributed to somatic or parallel induction of the germ-cells.

But let us leave Kammerer's experiments for the moment and consider what modern work has been done along the second line of attack—the transplantation of ovaries from one body to another.

In 1908 C. G. Guthrie * published a full account of experiments on hens. In each experiment a pair of hens was taken and their ovaries were reciprocally exchanged. In a number of cases fertile eggs were produced, which gave offspring showing the characters of the foster-mother. From this Guthrie concluded that there had been a modification of the implanted germinal tissue by the soma. The case was reinvestigated by Davenport † (1910), who concluded that Guthrie's experiments were unreliable for two main reasons. In the first place, the hereditary potentialities of the stocks from which the experimental animals were taken were not fully known. This knowledge is of course a necessary preliminary to any such experiments, so that there may be no doubt that a new character in the offspring is due to induction of the germ-cells and not to the appearance of a trait latent in the race from which the transplanted ovary was derived. Secondly, it is extremely probable that the eggs produced by Guthrie's hens were derived from regenerated ovarian tissue and not from a functional graft. For Davenport made six experiments on the lines of Guthrie's, in each of which the ingrafted ovary degenerated and eggs were produced by the regeneration of the original ovarian tissues. Further, the new ovaries should never be grafted in the places from which the original ones have been removed. Guthrie failed to take this precaution, so that it would be impossible to decide from a *post-mortem* examination whether the ovary present was graft or regenerated original.

* *Journ. Exp. Zool.*, Vol. 5.

† *Proc. Soc. Exp. Biol. Med.*, Vol. 7.

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A series of really critical experiments have been made by W. E. Castle and J. C. Phillips,* the first results of which were published in 1911 and a further case last year. In all, 141 female guinea-pigs were castrated, and into their body-cavities were transplanted ovaries of other individuals. In all cases the exact genetic constitution of the races to which the animals belonged was known. In seven of the operated females ingrafted ovarian tissue was demonstrated *post mortem*, and three produced young after having been mated with males. These three cases furnish the results of the investigation. In eleven animals original ovarian tissue was regenerated and in eighty-seven others no ovarian tissue at all was found *post mortem*. We will summarise the last published results only, that is to say those derived from the third successful graft which gave young. The conclusions from the two earlier successful experiments were exactly similar.

The ovaries of a *light cinnamon* guinea-pig were transplanted into a previously castrated *brown* female. In the families of both of these albinos occurred as recessives. The brown female was later on mated with an *albino* male. The expectation from the cross was as follows. If the mother were cinnamon, cinnamons and albinos potentially cinnamon should be produced. If the mother were brown, browns and albinos potentially brown would be expected. Actually five young were produced, of which two were light cinnamon and three albinos. A subsequent breeding test with one of the latter proved it to carry cinnamon.

The eggs from which this litter was produced were derived from the ingrafted ovary, which was thus shown to have been absolutely uninfluenced by the foster-soma. All three successful cases gave this conclusion, which, as Castle points out, does not prove that foster-mother influence is impossible. It is, however, unimpeachable evidence that in this critical case the implanted ovary was quite unaffected by the new soma.

As a continuation of his investigations, which have been summarised above, Kammerer also made experiments on ovarial transplantation. These experiments with salamanders gave in many ways remarkable results, and ones which are quite at variance with those of Castle and Phillips.

* *Carnegie Inst. Pub.*, No. 144.

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Kammerer states that there was never a regeneration of ovaries after they had been removed. This was tested by keeping a number of castrated females separately and subsequently examining them for ovarian tissue. It is not stated, however, whether the actual animals used in the experiments were examined *post mortem*. There was a striking difference in behaviour according as the colour varieties used were from nature or had been artificially produced. In varieties from nature the ingrafted ovary was quite *uninfluenced* by the foster-soma. For example, bilaterally symmetrical striped females, bearing the ovaries of irregularly spotted, when crossed with spotted males always gave spotted young. That is to say, the bilaterality in the markings of the foster-mother did not appear in the offspring. On the other hand, when the bilaterally striped foster-mother was an animal in which the markings had been produced under the artificial conditions of the induction experiments detailed above, she *did* influence the ovary ingrafted into her. In this case bilaterally marked females, bearing the ovaries of irregularly spotted, when crossed with spotted males produced bilaterally symmetrical striped young. The characters of the foster-soma had impressed themselves on the grafted ovary.

Whether we accept this result or prefer to wait a further demonstration, we are compelled to question the teaching of Weismann regarding the total independence of the "germ-plasm." Are the ovaries and testes or the cells in the developing organism which will later give rise to these structures really physiologically distinct from the rest of the body? In a sense, of course, it was never implied that they are *absolutely* distinct. The germ-cells are nourished in the same way as other cells of the body and their waste products are removed by the same paths. This type of interdependence receives visible demonstration by the experiments of Sitovsky* with moths and Riddle† with tortoises and fowls. The animals were fed with a certain dye which had the property of staining fat. This dye coloured the fat both in the somatic and germ-cells, and through the latter was transmitted to the offspring, the fat of which was also stained. Again, the effects of a poison

* *Bull. de l'Acad. des Sci. Cracovie*, 1905.

† *Journ. Exp. Zool.*, 1910.

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administered to an animal are seen not only in the soma but in the germ-cells as well, through which the effects may be manifested in the descendants. C. R. Stockard and Dorothy Craig (1912)* fed guinea-pigs continuously with alcohol. They found that from forty-two matings of such animals only eighteen young were born alive and of these only seven survived more than a few weeks. Nine control matings with untreated individuals gave seventeen young, all of which grew to be vigorous animals.

Investigations made by Ignaz Schiller in Paris (1912 and 1913)† were designed to touch our present problem more closely. They were intended as a preliminary investigation into the question of the possibility of somatic induction to test whether injuries to external parts of the body have any visible effect on the germ-cells. Frog tadpoles (numbers not given) were operated on by amputating the tip of the tail with a red-hot needle. Most of these operated animals were found to show abnormalities in the mode of cell-division in the germinal tissue. Such abnormalities were not present in unoperated tadpoles. The operated animals were examined for pathogenic bacteria, to be sure of no infection of the germ-cells in this way, but always with negative results. The possibility of poisoning through breakdown products of the protoplasm at the point of injury seemed unlikely, since the operated individuals continued to live. Further, the posterior limbs of frogs were gradually amputated by tight ligaturing with catgut. Many of the eggs in the ovaries of females operated upon in this way were found to be degenerate. Later experiments were made in the same way with white mice, which were killed twenty-four to thirty-six hours after the operation and their ovaries sectioned and examined.

These experiments were intended to prove that germ-cells are more easily affected by external influences than are somatic cells. But apart from the statement that in the tadpoles "all other organs . . . showed themselves to be absolutely intact" and that, in the mice, the germ-cells showed degeneration while the connective tissue cells of the ovaries did not, there is no statement of any comparison between the effects on the somatic and germ-cells. Apparently no such accurate comparison was made; but even if it

* *Arch. f. Ent.-mech.*, Bd. 35.

† *Ibid.*, Bd. 34 and Bd. 36.

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had been shown that injury to an extremity causes degeneration processes in the germ-cells, it is difficult to see how this knowledge could affect the question of the inheritance of somatic modifications. For the production of abnormalities is a very different matter from an induced change in the hereditary qualities of the eggs or spermatozoa.

Along a similar line, however, experiments are being made which give promise of shedding considerable light on the problem. A number of workers under Hans Przibram in Vienna have been making investigations to test the extent to which external stimuli incident on the soma can *directly* reach the germ-cells. The experiments which most interest us here were published by Šečerov in 1912 * and deal with the proportion of the light falling on the body of a salamander which can penetrate to the ovaries. Briefly stated, his method of procedure was as follows: animals were castrated, and in the position where the ovaries had been were placed small sealed glass tubes containing sensitive photographic paper. Light of known intensity was then allowed to fall upon the salamanders for a definite length of time. After this the glass tube was removed again and the amount of darkening of the photographic paper noted. The result was that the paper was always affected by the light, and a calculation gave the average proportion of the incident light which penetrated to the position of the ovaries as 1/173.

It has already been pointed out that in order to be certain that an external factor has affected the germ-cells by somatic induction, the possibility of a direct action must be eliminated. In consequence, the experiments of Šečerov have a special bearing on the interpretation to be placed on Kammerer's work. They suggest that the effects of the yellow and black grounds on the hereditary qualities of the germ-cells may have been *direct*, the light influencing the soma and germ-cells at the same time by parallel induction. Nevertheless we must remember that Kammerer showed that the colour stimulus works by way of the eyes and nervous system, not directly on the pigment cells, which makes it seem improbable that it could act directly on the germ-cells.

From the foregoing it is plain that the problem of the possibility

* *Arch. f. Ent.-mech.*, Bd. 33.

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of the inheritance of somatic modifications produced as a reaction to the environment is as yet by no means solved. Nevertheless, we realise better to-day the meaning of the problem and the conditions under which it must be attacked, and a considerable body of evidence *pro* and *con.* is being accumulated. Few biologists would be found to maintain the view that the transmission of somatic modifications is one of the chief means by which the characters of species become altered ; it is possible, however, that it is one among others of the influences concerned. It is plain that if changes in the hereditary factors of the germ-cells, brought about either by somatic or by parallel induction, are to leave their mark on the characters of the race, they must be permanent. No somatically induced changes have as yet been shown to be permanent in later generations—Kammerer did not test this point by keeping the offspring of his modified salamanders for several generations under normal conditions.

What is wanted most at the present time, however, is one really undoubted case of the transmission of somatic reactions. This has been clearly pointed out by Bateson * in his recent volume. He observes that no two workers have as yet confirmed one another over the same ground, and that the multiplication of examples is of little use until one case has been thoroughly investigated. When we have before us undoubted proof that somatic induction is capable of affecting the inheritable characters of an organism, then we can start to investigate the extent to which this factor is of importance in changing the characteristics of a species in response to a changing environment.

In conclusion, a word should be said about a very curious point of view which is always to be found cropping up in discussions on acquired characters. Stress is laid on the fact that it is difficult or impossible to imagine how a modification in a peripheral part of an organism could be reflected on the germ-cells and there influence the factors controlling the development of the modified part in the next generation. Of course it is impossible to imagine. When we know absolutely nothing about the nature of these presumed hereditary factors in the germ-cells, and practically nothing

* *Problems of Genetics*, 1913.

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of the means by which one part of an organism influences the other parts, it is not surprising that the mechanism of a possible somatic induction is unimaginable. What we are concerned with at the present time is whether there is such a phenomenon as somatic induction at all, and if so to what extent it takes place. The mode of its action, which will certainly be found to be physico-chemical and not by means of any morphological concepts like "pangens," will better be left to a later generation understanding considerably more than we do of the physiology and chemistry of the living organism.

THE MILK PROBLEM

By John J. Buchan, M.D., D.P.H.

THE difficulty of arousing public enthusiasm for the reform and control of the methods of dealing with milk probably explains how it is that one of the chief foods of the people is at the same time the most dangerous and sophisticated. At present sanitary authorities generally are but poorly endowed either with the powers or the spirit of reform, and what is most wanted, is a well-informed public opinion, which will not only demand new and more drastic powers for the authorities, but also insist upon these powers being fully put into force.

Of all foods milk is the one most easily contaminated, yet under the usual conditions of supply it is contaminated at every stage from its source to the home of the consumer. The cleanliness of a sample of milk may be roughly determined by passing it through little discs of cotton wool or surgeon's lint, as described in the excellent article of Dr. Eric Pritchard in *BEDROCK* of April, 1913. The depth of staining of the disc gives a fair idea of the amount of dirt present in the milk. This dirt consists chiefly of *débris* got from the unclean hands of the milker, hair from the cow, and vegetable fibre from the dung on the udder and flanks of the animal. In a carefully prepared milk such deposits will not be found; in any case, they could easily be removed by filtration after milking, but this is a practice which is rarely carried out efficiently by the milk producer. The most reliable means of ascertaining the care with which milk has been prepared and handled is by enumeration of the number of micro-organisms in a cubic centimetre of the sample. Milk examined bacteriologically compares most unfavourably with any other food, indeed sewage itself will often give a better bacteriological count than some samples of town's milk. This is due to two causes :

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first, the carelessness in its preparation, transit and storage already referred to ; and, second, the fact that milk itself forms an almost ideal medium for bacterial growth, and any effective attempt to retard the multiplication of bacteria in milk is a rare exception in the milk industry. The bacteriological count gives us the product of the contamination and the faulty storage of milk, and therefore may fairly be taken to represent its comparative wholesomeness. In saying this we are for the moment excluding milks which may contain specific disease-producing organisms like the bacillus of tuberculosis or of typhoid fever.

The following table compiled from the health report for 1912 of the City of Baltimore shows the extent of the bacterial contamination of milk as it is distributed in that city :—

Place of Collection.	Number of Samples.	Number giving less than 50,000 Bacteria per cubic centimetre.	Worst Sample. Number of Bacteria per cubic centimetre.	Best Sample. Number of Bacteria per cubic centimetre.
Stations	3,066	1,363	64,000,000	10,000
Wagons	582	103	150,000,000	20,000
Stores	230	72	480,000,000	20,000

These figures from Baltimore do not differ in material respects from figures got by examination in many other localities both in this country and in the United States. It may be stated at once that it is practically impossible to obtain from the cow a milk free from bacteria, but such grossly excessive numbers are beyond anything that the consumer can reasonably expect in his demand for new milk. There is, indeed, a great need in this country for a beginning to be made in setting up a bacteriological standard for new milk. This has already been done in many of the cities of the United States ; in New York, the standard adopted in 1900 was 1,000,000 bacteria per cubic centimetre, and at the end of 1912 Baltimore adopted a standard of 500,000 per cubic centimetre.

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In commenting on standards, Eastwood * points out the great difficulties attendant on their enforcement when as is usually the case the city milk contains several millions of bacteria per cubic centimetre, but he states that the results already obtained in America by the use of bacteriological methods are worthy of admiration. There can be no doubt that even better results would accrue in this country were a bacteriological standard set up and routine bacteriological examinations more frequently practised. It is not suggested that such standards could, in the first instance, be rigidly enforced, but if the power of establishment of such standards were given to local authorities in this country, with, say, the consent of the Local Government Board, and a beginning made in each district by the adoption of one not too exacting at first, local authorities would have in their hands a powerful lever for dealing with the dirty and careless milk producer, or dealer, and by the raising of the standard from time to time a gradual reform could be brought about. It is admitted that in order to be able to conform to any reasonable bacteriological standard new methods would require to be introduced by most farmers and milkmen in their business ; but this is the end sought, and the only practical objection that may reasonably be taken is that these new methods would so increase the cost of production that prices would rise, so that milk, the most important article of food we possess, would be out of the reach of great numbers of the people. This is an argument that requires careful consideration, and it has apparently derived an added force by the efforts of some well-meaning wealthy philanthropists in the United States and in this country who have personally incurred very large expenditure in showing how a pure milk could be prepared—without any adequate financial return. There can be no doubt that prices would rise with the production of a purer milk, but such a rise in prices need not be great and could be kept within reasonable limits by lessening the number and enlarging the size of the individual businesses, or by co-operative arrangements between farmers, for much of the plant necessary can deal with large volumes of milk. If it is to make real progress in the

* Report to the Local Government Board on American Methods for the Control and Improvement of the Milk Supply, 1909, New Series, No. 1.

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production of pure milk, the milk-producing industry, like many other industries, must be organised on a large basis, or it is likely to fail.

The experience of the United States in the commercial possibilities of what is called there certified milk, or a pure milk certified after careful inspections and tests by a milk commission, is on the whole not very encouraging to the milk producer in this country who is in the business to earn his living. But the experience there should not be regarded as a criterion, for, by the establishment of certified milk and ordinary milk, the objectionable practice of selling milk graded as to its cleanliness is, in effect, brought into existence. The consumer is asked to pay a little more for a commodity, the advantage of which does not appeal to him at the time of sale, and his commercial instincts are usually keener than his health anxieties. It is to be feared that any system of grading milk either as to cleanliness or quality would in this country ultimately benefit more materially the dealer in the inferior article. If the milk supply as a whole be attempted to be improved we can anticipate not only a greater public health good, but a greater commercial prosperity of the better milk producers. Nevertheless it is to be noted that one effect claimed in America for certification of milk is that it has formed an important factor in improving the general milk supply, and "that it is possible to buy in New York City milk having a bacteriological content not materially exceeding that of certified milk at a penny a quart more than the ordinary farmer's product." *

In stating the enormous number of bacteria which raw milk usually contains it is not suggested that these organisms are disease-producing or pathogenic. The great majority are harmless to man, but there is no good ground for believing that the presence of any bacteria in milk is either necessary or beneficial. All bacteria found in milk must be classified with other added and potentially dangerous impurities, for any supposition that a kindly Nature had provided that the cow should add bacteria to its milk for the human infant's good is unthinkable. We, on the other hand, know that a large amount of the milk distributed in the towns of this country

* Buckley, *Public Health*, January, 1914, p. 125.

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does contain disease germs, and in these cases the bacterial count forms no index to the wholesomeness of the food. The pathogenic organism most commonly found in milk is the bacillus of tuberculosis of the bovine type, and the usual cause for its presence is tuberculosis of the udder of the dairy cow. The frequency with which this organism is found in samples of town's milk varies with the efficiency of the veterinary inspection of the district of supply. Of milks coming into Bradford during the past two years about 10 per cent. were tuberculous, and even worse experiences are not uncommon in other cities. It is now an accepted fact by all reasonable observers that tuberculous milk is a very important cause of many kinds of tuberculosis. It is quite possible, it is true, to drink occasionally tuberculous milk and not suffer from the disease, but it is not permissible to draw from this the conclusion that tuberculous milk is innocuous. The accumulating results of scientific research in recent years into tuberculosis of the glands, bones and joints in man, particularly the work of Stiles, Fraser, and Mitchell, afford most valuable positive evidence of the danger of tuberculous milk. Mitchell,* in a careful examination of seventy-two cases of tuberculous cervical glands in children, found that in sixty-five the bacillus was of the bovine type, while in 84 per cent. of the cases, two years of age and under, the food of the child had been unsterilised cow's milk from birth. He records in detail several cases where the relationship of tuberculous milk as a cause, and the occurrence of tuberculosis as an effect, appears to be quite obvious. One case coming under his observation is very instructive, and worth quoting. A baby of nine months, fed on unsterilised cow's milk, was operated upon for multiple osseous tuberculosis, and the bovine type of the bacillus of tuberculosis was found. The father and mother and the other children were all healthy. An investigation showed that in the small dairy farm supplying the household with milk two of the six cows kept had tuberculous udders, with the milk from both teeming with tubercle bacilli. With such evidence as this we can afford to neglect a very large number of animal-feeding experiments giving negative results.

By more frequent and thorough veterinary inspection of dairy

* *British Medical Journal*, January 17th, 1914, pp. 125 *et seq.*

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cattle, with the application of the tuberculin test, and the slaughter of affected animals, tuberculosis could in time be eradicated from dairy herds, and in this country the first step towards this end was made possible last year by the issue of the Tuberculosis Order of the Board of Agriculture and Fisheries.

Besides the tubercle bacillus numerous other pathogenic organisms are found in milk, some of which have come from the cow itself, while others have been added by affected or other persons handling the milk. From this latter fact it is clear that not only is veterinary inspection of the animals needed to prevent diseases being spread by milk, but the most careful medical attention must be given to all persons associated with a milk supply. It is not possible for a local authority to so medically inspect all those brought into relationship with milk sold that the authority can ensure absolute freedom from the spread of disease by this cause. Outbreaks have been recorded where persons whose association with the milk supply was most remote were able to spread disease by this means, and to more effectually prevent such occurrences the further education of all persons interested in the trade seems to be a primary necessity.

One of the most serious of bacterial contaminations of milk occurs in the home of the consumer. It is not difficult to realise how this takes place when one considers the manner in which milk is kept in the majority of houses. However pure a milk may be when delivered, unless it is properly protected, and its temperature kept at a low level, a most dangerous contamination will ensue. Newsholme,* fifteen years ago, gave good reasons for believing that summer diarrhoea arose from the infection of the milk while stored in the consumer's house. His conclusions have on the whole been generally accepted. Delépine, however, as a result of his investigations appeared to lay stress on the infection in the cowshed as the cause of summer diarrhoea, but he was not able to exclude infection in the home. It is a matter of common experience in this country that outbreaks of summer diarrhoea are seldom associated with any one source of milk supply, and it would seem clear from the epidemiological work of recent years that the main means of spread of this disease is by infection of the milk while stored in the house,

* *Public Health*, 1899—1900.

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the organisms being carried possibly by flies from sources of infection associated with the insanitary surroundings such as privy middens, and the like.

The methods of storage of milk in the home is not usually looked upon as a phase of the milk problem. It is, however, closely allied and equals, if it does not surpass, in hygienic importance the problem of obtaining for the people a pure milk supply. We must recognise its relationship to questions of housing, but much may be done in the manner of milk distribution to lessen the evils of home contamination. For this reason as well as for that of protecting the milk from careless handling prior to delivery, bottling of the milk at the place of production is necessary. Unfortunately bottling forms a relatively expensive item in milk preparation and this has been the main obstacle preventing its general adoption. In America, where the amount of bottled milk sold is proportionately much greater than in this country, a movement is now on foot to prohibit entirely the sale of churn or "loose" milk, and making it necessary for all milk to be sold in the "original or unbroken package."

The difficulties attendant on the reform of the milk supply, and the lengthened period which must elapse before it becomes possible for milk prepared under ideal conditions to be available, have led scientific workers to devise various means of rendering milk safe. Sterilised milk, pasteurised milk, and dried milk are now all commonly used with good results, while the simpler practice of boiling milk has increased greatly in recent years. It is clearly recognised by all who have advocated the use of such methods that a pure milk is better than a purified milk, but until such a pure supply can be got the milk must be rendered safe. The possibility, indeed, of ever having a pure raw milk which can be used safely by the people is doubted by many competent persons, and it is significant that in America, where so much has been done in the preparation of pure milk, the advocacy and practice of pasteurisation has been increasing greatly within recent years. Both in this country and in America the most careful students of the milk problem have recognised that there is no antagonism between the demand for pure milk and the adoption of means for rendering milk safe. To permit a demand for pure milk to develop into an attack on methods of rendering milk safe may be passed aside as merely an obsession.

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It is on the question of artificial feeding of infants that objections to pasteurisation or sterilisation of milk have been most strenuously urged. These objections have been elaborated with a considerable amount of ingenuity by Vincent,* but a sympathetic examination of his arguments leaves one with the impression that the case against sterilised or pasteurised milk is not yet proved. There is, without doubt, great need for further scientific investigation into the biochemistry and bacteriology of milk, and the present time is one rather for the search for new facts bearing upon the question than for drawing conclusions from insufficient and too frequently faulty data.

It must not be forgotten that to the human infant, cow's milk raw or cooked is essentially a foreign substance. We have now reached a day scientifically when we have to credit our babies with biological and physiological attributes somewhat different from those of young calves. Large numbers of investigations show that the fluids of the human body present in their living state very subtle differences from the same living fluids in other animals. This is as true of milk as of blood serum, and it would appear that the more important biological properties of human milk cannot be replaced with advantage by those of cow's milk. Indeed, there is some reason to believe that the biological properties of cow's milk are actually harmful to the human infant. Dr. Janet Lane Claypon,† whose valuable work on this subject is worthy of the most careful study, in summing up her final report, states "that the weight of evidence suggests the absence of any direct value in the biological substances *per se*," and that "it also most decidedly shows the paramount importance of providing breast milk for the young animals." The best clinical experience in this country bears out the truth of both of these statements. It is to be remarked that were the feeding of infants with sterilised or pasteurised milk so frequently attended by the bad results alleged such a condition of affairs would surely sometimes have been noticed by many of those who have had a large experience of feeding by these means.

* *The Nutrition of the Infant*. London, 1913.

† Reports on Public Health and Medical Subjects to the Local Government Board, New Series, No. 63 and No. 76.

THE INSTRUCTION OF SCHOOL-CHILDREN IN MATTERS OF SEX

By Mrs. T. La Chard

THE present day consensus of opinion in favour of school education for the young is almost universal, and all classes and conditions of men in civilised countries readily admit that in every department of life specialised knowledge is becoming more and more necessary to success. Yet in spite of this, and of the growing comprehensiveness of education generally, one important branch of knowledge, viz., human sexology, is almost entirely neglected in schools, and as far as sex is concerned our children are left in childish and fantastic ignorance.

With the origin of this state of things we are not concerned in this paper, but rather with an enquiry into the need for change in this respect, which is being urged by many doctors, writers, educationalists and social workers.

The chief reasons brought forward in support of the movement for the sexual enlightenment of children are as follows :—

(1) It is urged that since education in its ever multiplying forms is assuming a much more important position in the moulding of the human being, it is absurd to omit from it the study of one of the most imperious human instincts, which underlies and is interwoven with so many human activities.

(2) It is stated on widespread medical authority that the dangers which beset the young child from the sexual side of life are much greater than were formerly supposed, and that the awakening of this instinct occurs much earlier in the life of the human being than has been so far recognised.

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(3) We are warned by expert facts and figures that in the interest of the individual and of the race some form of enlightenment is becoming increasingly necessary, for the avoidance of disease and of the physical and mental corruption in adult life, widespread through every section of the community and due, to a large extent, to ignorance of sexual dangers.

Taking these facts into account, it certainly seems advisable to introduce some amount of teaching in matters of sex into the curricula of schools, primary and secondary, and this addition must be welcomed by all, teachers and laymen alike, who believe that education should be a preparation for life. Here, at least, is one subject that cannot become useless or out of date. Educational theories may come and go, the classics may become obsolete and the Montessori method a thing of the past, but the sex instinct will remain one of the prime factors in the development of the individual and racial life of humanity.

Before setting forth certain proposals for sex teaching to the young, it may be useful to give a few historical references, and to mention some of the movements for enlightenment which are already in the field.

The first educationalist to recognise his responsibilities in this matter was Rousseau, who called attention to it in the fourth book of *Emile* (1762). In the same century a German reformer, Johann Gotthilf Salzmann, went a good deal further, and he may be looked upon as the first advocate of definite sex instruction for school children. He was the headmaster of a well-known boys' school at Schnepfental, and in 1785 wrote as follows:—

“ I firmly believe that at an early age children should be taught about the way they come into being. If there were a safe means of keeping them in complete ignorance of this matter, of preventing them from watching the pairing of animals, from thinking about the subject, or from hearing about it through playmates, servants or low company, I would express myself with more reserve, and would delay instruction in this matter to later years, when it will become a necessity, if the young man is not to risk both honour and happiness, owing to his lack of knowledge of cause and effect. But since it is impossible to prevent children from receiving teaching which will endanger the innocence of their hearts, I am determined to give it to them myself in such a way that their innocence will be safeguarded. I know that in giving such

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instruction a great deal has to be overcome. But, after all, what does the 'great deal' amount to? Prejudice and nothing else, and I believe that the gain will be inestimable."¹

It is difficult to realise that these words were written almost 130 years ago! Salzmann's friend and colleague—Basedow—was even more emphatic on the subject in his educational treatise, the *Elementarwerck* (1770—1774). The children were to be shown pictures of childbirth, and to be warned of the dangers of sexual irregularities from the very first. Boys were to be taken to hospitals in order to realise the horrors of venereal disease, and to protesting parents and teachers he quotes largely from the Old Testament in support of plain speaking on sexual matters.² But both Salzmann and Basedow were too much in advance of their times to exert much influence, and it was not until towards the end of the last century that the matter came again into prominence.

It is in Germany that during the last ten years the most thorough, scientific and sober work has been done. The Society for Combating Venereal Disease (founded in 1903) devoted its congress in 1907 entirely to sex pædago^y. The Ministers of Public Instruction for various German States were represented, and the meetings resulted in a more widespread sense of the importance and complexity of the subject, and the need for collecting the joint evidence of doctors, clergy and teachers. A natural history conference held at Stuttgart in 1906 authorised the publication of a memorandum which strongly advised the giving of sexual instruction towards the close of the school period. And, since the majority of teachers did not know how to set about their new task, some simple manuals were suggested giving graduated matter and methods for dealing with the same. Sex instruction has not yet been included in the official syllabuses of German State schools, but several towns have arranged for optional courses to be given to the pupils of their highest classes by carefully selected doctors, and this method of dealing with the matter is gradually making its way into the elementary and continuation schools. The scientific discussion of sex questions, and of sex pædago^y in particular is further encouraged by the Society for the Protection of Motherhood (*Verein fuer Mutter-schutz*), whose publications and activities are most numerous and useful.

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“Germany may not be a greater sinner than other lands, but it far excels all others in careful statistical studies, and its specific movement to have definite instruction in sex rests upon a wave of new interest and insight, which at present seems to bear some promise of radically reconstituting present ideas.”³

It is noteworthy too that Germany alone has produced a dramatic study of adolescence on realistic lines. In *Spring's Awakening*, Frank Wedekind sets before us a group of schoolchildren, just past the age of puberty, and works out grimly, but consistently, what suffering and degradation may fall to their lot as the outcome of sexual ignorance. As a play it is unique in the annals of dramatic art. In Germany it has become one of the regular stock of plays acted in the most eclectic theatre in Berlin. In book form it has gone through twenty-six editions and has been translated into various European languages.⁴

Meanwhile, in other countries, too, a sense of need for reform exists, and small but rapidly growing groups of doctors, educationalists and social workers are organising practical agencies for dealing with the difficulties. It is most important and valuable that so many different interests should be focussed on the question, since it is only by co-operation and sympathetic interchange of views and experiences that the final solution will be reached for the benefit of the community.

The consideration of the subject falls quite naturally under two headings :—

- (1) Who is to give the instruction ?
- (2) When and how is it to be done—the questions “when” and “how” being so intimately connected, that it is impossible to separate them for purposes of enquiry.

(1) Who is to instruct children in matters of sex ? Three factors are available in the solution of this part of our problem : parents, teachers and doctors. Hitherto it has been generally assumed that the home is the place where knowledge of sex (if any is desirable) should be imparted, and that the mother is the ideal teacher. This is no doubt true under natural and happy conditions, but as things stand to-day there are many cases in which the mother is not the most desirable exponent. She, herself, if a middle-class woman, has very often been brought up in conven-

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tional ignorance of sex matters, and is quite ready to let her child grow up in turn under the *régime* of silence or chance information which has apparently made no difference to her own career. And even if the home is indeed the best place for the instruction of middle and upper-class children in matters of sex, what of the homes where the mother is out at work all day, and where the children's nursery is the street?

There are, of course, some few cases even to-day, drawn from the most divergent classes of society, in which the children have been told the truth by their parents, and it is interesting to note with what gratitude and satisfaction those children, now grown up, refer to this, and how it seems to form a subtle link in the friendship and confidence between the old and the young which should be far commoner than it is at present. For in every case the part which the parents are to play in enlightening their children must depend not even so much upon the moral and scientific qualifications of the parents themselves, as above all upon their personal relation to their offspring. If their children trust them and have complete confidence in them, then they too should trust their children and should be able to answer truthfully and simply such questions as they may ask about life and its origin.

The teachers of to-day are probably as yet no better equipped than the parents. Dr. Eric Pritchard, addressing the Child Study Society in May, 1912, admits that—

“in many ways they are undoubtedly better fitted for these duties; they are better educated, more intelligent, more capable of controlling the children, and more respectfully regarded; in the school there is less sentiment and more discipline. But to counterpoise these solid advantages, there is the very real objection that the teachers are not equipped with the necessary scientific knowledge to impart lessons of this kind with the confidence, assurance, and complete detachment which is so essential for the matter of fact reception by the child.”⁵

Certain it is, however, that if educational teaching in sex matters is to be given at all, the teachers themselves must be the first pupils. The training college course for all teachers will have to include sound and adequate instruction in sex pedagogy, which should impress them with the need for and value of this work, and should emancipate them from ignorance and prejudice.

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It has also been suggested that the medical profession be made responsible for the enlightenment of the children, even to the exclusion of parents and teachers, and in this connection the claims of the school doctor are strongly and rightly urged.

Personally it seems to me, for reasons to be shown in the pages following, that all three agencies might well be enlisted in this service to the young, without the slightest danger of contradicting or overlapping one another in their efforts.

(2) When and how is instruction in matters of sex to be given ? On this part of the problem, too, opinions are divergent. It is well known that the onset of puberty brings with it certain radical changes and sexual risks, which it were well to prepare for and avert if possible by timely teaching and warning. But here already the question arises : when is the right moment to give this teaching ? Irrespective of sex enlightenment at home or in school, probably most boys and many girls have knowledge of the sexual processes, though this knowledge may be the very reverse of scientific fact. An interesting piece of evidence is given by a German elementary schoolmaster who, after giving a lesson on human sexology to a top class of boys and girls, asked them—(1) which of them had known something of sex before, (2) who had been their informants, and (3) at what age they had been told. The answers were as follows : Out of 28 boys 21 knew something about sex, and out of 28 girls 24. The boys had been told in one case by a father, in 4 cases by servants, in 11 cases by coachmen, and in other cases by a miner, a gravedigger and a beggar. The girls had been told in 2 cases by their mothers, in 6 cases by youths, in 3 cases by girl friends, and in other cases by an old beggar woman, a strange man, an aunt, a sister and an uncle. In 3 cases boys of 8, 9, 10 and 11 had been told, in 5 cases boys of 12, and in 4 cases boys of 13. The ages of the girls were not given.⁶ It is evident that information coming in so many cases from such unsatisfactory sources can hardly be called sexual enlightenment or sex teaching.

At this point it becomes necessary to define clearly what we mean by "sex teaching." It is assumed by some to mean teaching on all matters of sex generally, beginning with the processes of reproduction among the lower animals, and passing on to those of birds and mammals (including human beings) and dealing finally

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with the subjective problems of sex in the individual life, and the dangers and diseases connected with it. Others again would include only the latter facts under the head of sex teaching. It seems to me that we must carefully distinguish between the two sections of our subject, namely : (1) biological instruction which comprises elementary and wholly objective teaching concerning the natural processes among animals, and by implication among man ; and (2) sex instruction which deals with human sex physiology and hygiene, and the dangers of sexual irregularities and diseases. Throughout the remainder of this paper these two sections of the subject will be alluded to as "sex biology" and "sex hygiene."

It has been suggested that sex biology should be taught at a very early age to be followed closely by teaching in suitably graded sex hygiene, in order that all dangers may be avoided from the very first, the chief of these being, as far as elementary infant school children are concerned, violation and habits which may later lead to self-abuse. Now no amount of teaching in sex hygiene will protect a small child from being violated ; this is a matter for watchfulness by parents or responsible guardians of the children, or if necessary for better policing. Undesirable habits again, in the case of young children are often initiated by local uncleanness, irritation, etc., and much can be done to prevent their arising, by insistence upon scrupulous cleanliness at home and careful supervision in class. And above all they furnish a strong plea for condemning the whole of our elementary infant school system, which forces hundreds of thousands of children between the ages of four and six to sit still for hours at a time on hard wooden seats with little or no outlet for their overbubbling energy. An enlightened headmistress of one of the best, because most unscholastic, infant schools in London remarked that "children who are kept thoroughly clean and are allowed as much varied exercise as they require, rarely practise self-abuse."

But though it is probably true that most sound and healthy children reared under fair conditions at home and in school show no distinct signs of nervous sexuality, we must be prepared to meet many seemingly abnormal mental and physical states, even in quite early childhood, which Freud⁷ attributes to the awakening and operation of the sex instinct. As symptoms of such conditions he

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quotes extreme shyness, nail biting, grimace making, and the desire to expose themselves which may occur in children of five or six. For such difficulties no sex hygiene has any solution to offer, and tact and sympathetic intuition must still be called to our aid, as in every case where we are dealing with children.

Sex hygiene, then, is no subject for the lower classes of a school. It belongs to the onset of puberty, and should be taught to leaving classes, to the highest form of secondary schools and to all girls before they leave the elementary schools, and it is at this stage that the teaching should, where possible, be in the hands of doctors who alone can speak in these matters as those having authority. It is probable that with very little experience and special study the school doctors might become experts in the imparting of sex hygiene. Sex biology, on the other hand, is only an amplification of nature study, in the teaching of which many teachers to-day are trained to be experts. It is to them, therefore, and to the more enlightened parents of the future that we must look for help in the earlier part of the work.

The answer as to when the said biological instruction is to begin is not quite so simple. Probably it will vary considerably according to the social class of the children with whom we are dealing, and even more according to whether they are town or country dwellers.

At the beginning of the school age the town child is far more alert and curious, while the country child, on the other hand, is more familiar with fundamental biological facts, since the observation of animals cannot fail to make the elementary ideas of sex life clear to all. When the cow calves, the whole family in the country takes a deep interest in the event, and the intimate relations of man to Nature, and the very dependence of human prosperity upon natural fruitfulness in the plant and animal world makes the country child look at these things far more simply and naturally.

These observations may give us some hints of the lines on which the earliest biological instruction should be given to the little ones. The large facts of natural history rather than those revealed by the microscope are required first, and zoology would probably prove a better introduction than botany. In the lowly aquatic forms, and still more in insect life, we have great stores for observations of this kind. Beginning with the lower animals, instruction would

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gradually proceed to birds and end with mammals. Man need not be mentioned at all at this stage, or merely by the remark that he, too, is a mammal, and that his functions are similar to those of the higher mammals. Salzmann in the eighteenth century advocated teaching as follows at this stage : " The teacher should procure the pregnant female of some small mammal, *e.g.*, a mouse or squirrel, should kill it and opening the body show to the children the position of the young inside it, which will probably be still alive." ¹ Though interesting as an example of the thoroughgoing methods of the author, it seems too crude to be transplanted into the schools of to-day, where the teaching of kindness to animals forms a desirable part of the curriculum.

In answer to those who say that biological instruction is not satisfactory even at this stage without actual reference to the human being as a subject for observation, I would reply that they have too little faith in the common sense of children. Schoolchildren are required to perform, and actually do perform, far greater feats in the way of the association of ideas than are required for deducing the sex life of man from that of animals.

It is probable that the first teaching in sex biology should be given during the period of early childhood as a part of that Nature study which is begun, perhaps somewhat prematurely, or at least with a wrong choice of observational material, in the infant school or kindergarten, or at all events in the lowest classes of schools. It is at this time that the children most often begin to ask what nurses call " tiresome questions," and must, therefore, be satisfied or put off with certain facts or fairy tales. Lessing, in his *Erziehung der Menschheit*, advocates that children be given even at this stage " truth, nothing but truth, though not the whole truth," and I confess that to me the idea of conveying facts by fairy tales, which is frequently recommended in educational circles, seems absurd. Not because, as sentimentalists would have it, it undermines the subsequent faith of the child in its informant when at a later age the ruse is bound to be detected, but because the real facts are as wonderful as any fairy tale, and make a far more direct and forcible appeal to the childish imagination.

Two amusing stories of what intelligent children themselves think of these fairy tales are told by Dr. Karl Jaffé in his pamphlet

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dealing with the present state of sex instruction in German schools :—

“ An aunt from town asked her ten-year-old niece in the country whether the stork had been to their house again. ‘ The stork does not come to us here in the country, we get our children ourselves,’ replied the child. And a twelve-year-old boy, whose younger sister was asking him about the stork, replied in a superior tone : ‘ Do you really believe that the stork brings children ? A human being is a mammal, and the stork could not possibly take round young ones to all the different mammals.’ ”⁸

It looks almost as if the children themselves would, in the near future force us to abandon our unscientific methods.

Assuming, for the present, that sex biology, not treated as a separate subject, but as a branch of general biology, can form a satisfactory introduction to the fuller understanding of the subjective sexual problems, we now pass on to the question of instruction in sex hygiene, to be given to boys and girls at, or slightly before, the age of puberty. Puberty brings with it certain important changes, heralding sexual maturity in both sexes, but these changes come in radically different forms to boys and girls. It is, therefore, clear that though children of both sexes may have learned the elements of sex biology side by side in the same class, we shall have to differentiate considerably between them when we begin to teach sex hygiene.

Moll advises that the sexual enlightenment of girls should always be kept a little ahead of that of boys, as the development of girls up to the age of puberty is more precocious than that of boys.⁹ Having attained that age, the girl is usually less keenly and definitely conscious of her sex nature than the boy, and the risks she runs from sexual ignorance are quite different from his. The first teaching in sex hygiene to be given to girls should most certainly be preparation for the menstrual flow, of which the majority of girls are ignorant until it comes upon them, sometimes with disastrous results to their bodily and mental health. Tilt stated that from a statistical enquiry regarding the beginning of menstruation in nearly 1,000 women, he found “ 25 per cent. were totally unprepared for its appearance, 13 out of the 25 were much frightened, screamed or went into hysterical fits, and that 6 out of the 25

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thought themselves wounded.”⁹ The teaching on this particular point may be extremely simple. The girl need only be told that just as certain regular functions take place every day, so, during the mature part of her life, beginning at puberty, there will be other periodic functions due to more complex causes. These causes need not be dealt with at this point, if this is considered undesirable, more especially as there is still such great uncertainty as to the exact function of menstruation.

Stanley Hall in his exhaustive work on *Adolescence* advocates that girls, instead of shame, be taught “the highest reverence for this function”¹⁰ and that the first teaching on motherhood should be associated with the beginning of puberty. This seems to me a dangerous matter, leading not infrequently to the over-direction of the girl’s feelings to motherhood, which may induce a form of sex perversion. Several women have informed me that they experienced their first sexual thrill at the sight of a tiny toddling child, so much had the coming of the child been insisted upon, to the exclusion of all other phenomena of the human sex life. More particularly in the case of middle-class girls, anything that tends to overstimulate the sex feelings should be avoided at this age. Preparation for motherhood is no doubt desirable, but it belongs to a later age than that of puberty, when the girl is more familiar with her mature self, and is able to look upon the matter to some extent more impersonally. I should suggest that teaching on the subject of menstruation given at this time be extremely matter of fact, and that all sentimentality be barred, while, on the other hand, the hygiene of the function should be strictly attended to both at home and in school.

In introducing sex hygiene generally to the upper classes, it would seem advisable not to treat it as a subject by itself, but rather as a part of a course of instruction on general hygiene. Such a course would consider, *inter alia*, the care of the body, clothing, exercise, nursing, first aid, the dangers of alcoholism, etc. This teaching should tend to heighten a sense of responsibility in the young, and to familiarise them with the idea of taking a definite stand in the fuller life awaiting them when they leave school. To this general teaching more definite instruction in sex hygiene can then be easily added, such instruction being supplemented

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by the use of a simple scientific manual dealing with the physical, and moral aspects of sex life.

As regards the pathological aspects of the subject, viz., unnatural habits and venereal disease, which must certainly be included as an important part of the sexual instruction to be given to those leaving school, it is advisable to avoid all over-emphasis. Alarmist theories are never anything but harmful, since they are calculated to cause intense and often unnecessary suffering to sensitive and brooding natures, and to drive the less sensitive ones to further folly. More particularly in dealing with the difficult problem of self-abuse should the teaching be sane and moderate, and at this point it becomes increasingly clear why it is so desirable that the instructor should, wherever possible, be of the medical profession. For the lay teacher, anxious to give the greatest help and soundest advice to his pupils in this matter, is faced by the most bewildering and contradictory evidence. On the one hand, he is led to believe that masturbation, practised according to recent authorities^{11, 12, 13} by some 90 per cent. of schoolchildren of varying ages, is the root of all evil.^{14, 15} On the other hand, he is assured, likewise on strong medical authority that it is a strictly physiological activity,¹⁶ and that he is not to regard it as a vice.¹⁷ Finally, he is urged to dismiss all extravagant views "concerning the awful results of masturbation as due to ignorance and false tradition, though it must be pointed out that even in healthy or moderately healthy individuals it may produce results which, though slight, are yet harmful."¹⁸

Pending the more unified verdict of medical science, the would-be instructor of youth should confine himself to the very simplest and most unequivocal teaching in this matter. Probably the best line to follow is that of Dr. Heidenhain in his pamphlet dealing with the sexual teaching to be given to girls about to leave the elementary school. "The sexual organs are the highest and most delicate organs that the human race possesses. Now delicate organs, *e.g.*, the eye, must be most carefully handled, and only touched for purposes of cleanliness, and this is much more the case with the sexual organs."¹⁹

Teaching on the subject of venereal disease is far simpler, though here again all sensationalism should be banned. The instruction

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should include plain facts and figures, together with simple rules for prophylaxis, which we have reason to believe would be more effectual than any attitude of excessive sentimental horror. Lieut.-Colonel Gibbard, R.A.M.C., reporting before the Royal Commission on Venereal Disease concerning its decrease in the Army, attributed **such** decrease, partly at least, to the instruction of the men by lectures and individual talks. On the subject of education respecting venereal disease, he thought "that there would be great advantage in lectures being given at all large factories by selected medical men, or women, where the employees are women" (eleventh meeting, January 19th, 1914).

It is, perhaps, owing to the ardent hopes of social reform attached to this teaching in sex hygiene, that its advocates find it extremely difficult to express their opinions with reasonable moderation. In this country, at least, most of the popular pamphlets and books recommending some form of sex pædago^y are far too lavish of superlatives and of the rhetorico-sentimental method, though no doubt this will change when once we are clearer about our methods of handling the subject. At present these are and must remain largely tentative, since our lack of experience makes it extremely difficult to determine the wisest procedure.

It is for this reason too that I have omitted from consideration in this paper all detailed questions in sex pædago^y. The numerous and important problems of practical policy can be settled by the future alone, when we are far more familiar with our subject than is at present the case. Such questions which have yet to await the verdict of patient and unprejudiced experiment are, to my mind, *inter alia*, the problem of class *versus* individual instruction, married *versus* unmarried instructors, co-education or separation of boys and girls for purposes of sex instruction, the exact age for teaching, alternative methods of instruction, the problem of the rural school where it is impossible to obtain the school doctor for teaching purposes, etc., etc. At present the main object before us is to face the problem as a whole and to enlist the sympathies of the home, the school, the medical profession, the clergy and every other helpful agency in solving it for the benefit of the youth of our country.

But during the present experimental stage the most careful selection of teachers, medical and lay, and of books cannot be too

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strongly urged. Partisan teaching of any kind should be discouraged, for schools are not sexological debating grounds, and "the pre-coocious, onesided and often erroneous information on sex subjects now administered to young girls by frustrated or embittered persons for whom the word morality has only one application"¹⁹ is worse than the old method of silence. Would-be reformers of the marriage laws, neo-Malthusians and sex cranks of all kinds must find other audiences than the youth of our schools. Books such as the *Bar of Isis*,²⁰ *The Great Scourge and How to End It*,²¹ and some of the articles that appeared in the *English Review* during the autumn of 1913 are more dangerous to the reform of sex teaching in schools than the vulgar and feeble attacks of the halfpenny press as seen recently in connection with the Derbyshire village school case.

It may seem strange that religious instruction has not been mentioned until now as a factor in sex pædagogy. But, speaking personally, I see no other way to reform than that of natural science teaching, first in the form of biology, and at a later stage in that of hygiene. Religious instruction in the case of younger children should have as little as possible to do with sex, but the support of religion would be most valuable at and after puberty. It is difficult to see how children between the ages of six and twelve are to be helped by the repetition and formal explanation of the seventh commandment, while for young people who have already received sex instruction on a natural science basis, a religious and ethical motive for self-control would be of considerable importance. Needless to say that such religious teaching can only then be of value when it stands in definite relation to the actual conditions of modern life, quite apart from all unreality and formalism, which would only involve a vain appeal to youth.

Literature and art again are valuable agencies in sex pædagogy. "The greater part of literature is more or less directly penetrated by erotic and auto-erotic conceptions and impulses; nearly all imaginative literature proceeds from the root of sex to flower in visions of beauty and ecstasy."²² And this applies equally to art. Speaking at the School Hygiene Congress of 1907 the German headmaster Max Enderlin advocated that children be early familiarised with the representation of the nude in ancient sculpture

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and in the paintings of the old masters in order "to immunise them against the representations of the nude which make an appeal to their baser instincts."

Literature and art alone can supply the imaginative element in sex pedagogy and that but vaguely and quite indirectly. For the beauty and joy of personal love that glorifies life and seems to make eternity a working hypothesis, cannot be taught in school or out of school. To speak of it is to try to describe a sunrise to one who cannot yet see. All that must be left to manhood and maidenhood, and to the teaching of the love god himself, when he comes to take the place of the anxious instructor at the desk.

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DR. ARCHDALL REID ON "BIOLOGICAL TERMS"

By the Hermit of Prague

WHEN one finds oneself in disagreement with so fine and practised a thinker as Dr. Archdall Reid, and on his own especial subject, a plentiful lack of self-confidence is bound to be one's portion. Those who have the good fortune to be acquainted with his biological writings will not require to be told this. Still, as his article in the January number of *BEDROCK*, entitled "Biological Terms," contains several noteworthy and novel pronouncements, perhaps a few comments thereon may not be wholly out of place.

It should be understood at the outset, that I am not in any way reflecting on Dr. Reid's biological teaching, for which, indeed, my admiration is unbounded ; nor have I any desire to break a lance on behalf of the Mendelians or biometricians, whose champions ought to be able—it is curious, however, how they shrink from doing so—to try a fall with him themselves. No, my objects are four, and four only : (1) to defend such distinguished biologists as Wallace, Spencer, Romanes and Weismann from the charge of having used, and of having had their ideas of reality distorted by the use of, "unmeaning terms" ; (ii.) to show that Dr. Reid, though writing on "biological terms" himself, would appear never to have analysed the functions of a scientific terminology in even the most elementary fashion ; (iii.) to comment on and defend the traditional terminology Dr. Reid once used (and still uses, as will be shown, when off his guard) with such fine results, but has recently discovered to be a most grievous source of confused thinking (except on the part of Darwin !) ; and (iv.) to prove that Dr. Reid's proposed amendments, instead of facilitating comprehension, would really result in precisely the

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reverse. Of the noteworthy and novel pronouncements referred to,
take the following :—

“ I am still convinced that the statement that acquirements are transmissible is nonsense ; but I am equally convinced that the converse is also nonsense. At first these statements seem to have meaning, but they have none really—no more meaning than a statement that gravitation is blue or not blue, or that distance has or has not weight ” (p. 515).

m In view of the universally admitted fact (the keenest logomachist that ever lived would not overtly dispute it) that the **s** supreme rule governing the interpretation of a speaker's or a writer's **l** language is : Fathom the intended meaning ; this is, to put it mildly, **a** rather startling assertion ! The patent facts involved in bi-parental **r** reproduction (*pace* certain superstition-ridden Australian aborigines) **h** have been known and geared-up to the natural order of things, **f** from time immemorial. Our prehistoric ancestors must have been **a** as well aware as we are that a scarred and mutilated man *cannot* **b** beget a child that shall be similarly scarred and mutilated at birth. **A** And if, with innumerable instances of this kind in their minds, they **f** found it a matter of practical convenience to speak of them collectively as a class ; and if, with this in view, they gave them some **s** such roughly allusive name as “ gotten injuries ” ; and if, regarding **t** them, from the other aspect, they bestowed on them such an additional name as “ non-reappearable-at-birth,” who could say that **t** their terminology was in any way meaningless or misleading *to those acquainted with the facts ?* The word “ meaningless ” is here repeated with intention. For, fully to appreciate Dr. Reid's position, it must be remembered that it was not as an ill-chosen and misleading expression, and one that might well be improved on, he condemned the phrase “ acquirements are not transmissible.” Nothing of the sort. He made the unconditional assertion that no intended meaning of a rational order had ever been conveyed by it, or ever could be. For how else is his pronouncement that it has “ no more meaning than a statement that gravitation is blue or not blue ” to be interpreted ? And yet, it is, I believe, a fact that Dr. Reid's and my own ideas as to what, biologically speaking, *really happens in nature*, are perfectly in accord. Moreover, many of the most valuable of those ideas were imparted to me by himself, and by means of the very terminology he would now discard.

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But after making the sweeping asseveration quoted above, Dr. Reid seems to have had a certain revulsion of feeling. His faithful old retainer "transmissible" should not be dismissed from his service altogether. He should be found another job. And so, indeed, he has been. Through the kindly thoughtfulness of his old master, he is still to flaunt it in biological terminology. His incorporation in so striking and valuable a phrase as "non-existent characters are non-transmissible" will assuredly confer on him all the immortality such a really superfluous old person could possibly desire. It is true that Dr. Reid's advocacy of this phrase is not absolutely explicit. Nevertheless, it is impossible to misconstrue his meaning. For, on p. 530, we find "Now we are taking a step further; we are denying the very existence of inborn and acquired characters as such." But why did he not inform us of their non-existence at the start? For if, instead of saying, as he did at the commencement of his article, that the statement that acquired characters were not transmissible was nonsense, he had said that the statement that non-existent characters were not transmissible was nonsense, there would have been no more potther.

But, jesting apart, what, in the above context, is the meaning of "existence as such"? Does Dr. Reid think that the words "inborn" and "acquired" in their biological usage have fixed and definite "dictionary meanings" like, for example, "square" and "bitter"? If all bitter substances were incorporated in a single class, the word "bitter" *could* perform the double function of fully defining the particular attribute common to the class (class-definition), and of serving, also, as the necessary *class-name*. But this is only possible where the significative reference is both simple and definite. In the case of such a word as "inborn," however, the significative reference is both complex and indefinite. As to its complexity, think of the manifold, amazingly correlated, and protracted happenings it necessarily implies. As to its indefiniteness, there can be no more convincing evidence than its dictionary meaning (the Century's), which only runs to "innate, implanted by nature." It follows, therefore, that when "inborn" is used as a class-name, its other, and logically distinct duty of fully defining the attributes common to the class, can be but inadequately and perfunctorily discharged. In other words, "inborn" is quite

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ve as a class-definition, while at the same time, elements, limited though they are, render it admirably *class-indicator*. If this be so, Dr. Reid's denial of an inborn character *as such*, is void of all pertinence. Nobody can dispute the way or another. Therefore, nobody can dispute definitions and class-names. Moreover, any sound-able may be legitimately chosen as a class-name, *letter of the alphabet*. But as there is no descriptive burden of recollecting to which class-definition they peculiarly ineligible. As an example expected to reappear in the latter: "Those classification that must have commended itself to a member of the class had to be referred to have fallen on the letter B. This would mean time a kind would be indispensable. Let us say a character is B is quite without meaning. But that means in which a character can be both *called* by its class-name and the class-definition (as in the case of "square" it is, strictly speaking, only the descriptive element etc., have never been of identical significance with any class-definition they have ever been associated with. Consequently, to say a character is inborn (save in the sense that it belongs to a class all of whose members have been arbitrarily named inborn) is wholly indefensible.

And so it comes about that Dr. Reid and I are both agreed that to say a character is inborn is logically unsound. But though we have reached the same goal, we have reached it by absolutely

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different routes. He, through holding that the epithet "inborn," in its dictionary meaning, is intrinsically unsuitable as a predicate for the subject "biological character," and I, through perceiving its unfitness, as a class-name, to serve as a class-definition.

Of course, when Dr. Reid describes characters as combined products of potentiality and stimulus (nature and nurture), I quite understand what he means and am in full agreement with him. But abstract terms (like fire) are good servants, but shockingly bad masters, and, if they are not kept in steady touch with the concrete, are apt to lead their users into very queer places. Here is an instance :—

"How did it happen, then, that men like Wallace, Spencer, Romanes, and Weismann used unmeaning terms? The reason is plain in their books. Regarding all the characters of the normal individual as inborn, and every change produced by an easily observable cause as acquired, they never thought of characters as combined products of potentiality and stimulus (nature and nurture). In effect they supposed some characters were products of nature and others products of nurture. In other words, they assumed that an inborn trait was one which nothing evoked out of something, and an acquired trait one which something evoked out of nothing" (p. 528).

Now observe what tricks these highly abstract phrases, "potentiality and stimulus," "nature and nurture," have played on Dr. Reid's mind. They have positively made him believe, for the nonce, that those distinguished thinkers—Wallace, Spencer, Romanes, and Weismann—actually thought that some characters are products of nature and some of nurture, which, *when brought down to the concrete*, means, *inter alia*, that a scar could come into existence without any physiological activity on the part of the organism scarred, and that a child could grow up without being fed! Verily, to be charged, be it only implicitly, with being so crassly ignorant of "the systematic nexus of things" as that, is enough to make the remains of that great student of actuality, Herbert Spencer, shatter their cinerary urn. Of course, if Dr. Reid had proved that these famous biologists had formulated, implicitly or otherwise, nonsensical class-definitions (*e.g.*, all characters possessing scarlet scents), there would have been nothing more to say. But he attempted no such charge.

Perhaps the reader may imagine that the words "inborn"

and "acquired" are to be banished from biological altogether. They are to be reserved for especial pur- pose. Though not on any account to be biological characters themselves; to "likenesses and differences" (character-compounded) organisms they may when Dr. Reid perceived that the logical application of terminology in the state of Denmark "is past comprehension. Unless how it was he did not begin to suspect there was "something musical in the state of Denmark" is only used in biology at point, it must surely, be ridiculous to say there existed a musical pun- tual likeness between them! Of course, if Dr. Reid had realized that, on a class-name, "inborn" is only used in biology he could not have fallen into this trap. For change the class-name (which is all that can be done) can only be applied as such to biological characters what conceiv- able meaning the phrase "Blikenesses and differences" can have, —the class-name, therefore, that it gets from the original class-definition. It follows, that the definition for which B has been chosen as the symbol, B-character, is therefore, that two organisms can only be said to be different in each other when there exist corresponding B-characters in each other. For the actual fact is that to call the likenesses and differences between two organisms inborn would be quite meaningless, unless they each possessed characters on whom the class-name "inborn" had already been conferred.

On the first page of his article Dr. Reid declared himself convinced that the statement that acquirements were transmissible was as meaningless as would be the assertion that gravitation was blue, and later on he pronounced the idea of a character being inborn as equally nonsensical. And yet, in a footnote on p. 531, we find him, in a happy moment of forgetfulness, using these very self-same terms as effectively and as intelligibly as in the days of his ungeneracy! How could the Lamarckians be given a neater *coup de grâce* than this:—

"If the higher animals have evolved from lower types through the 'transmission of acquirements,' many characters which are inborn in the former should be acquirements in the latter.

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We have seen that low animals of the present day (and presumably those of former times) have little or no capacity to make use-acquirements. It follows that the higher animals cannot have arisen through the 'transmission of such characters.' "

Here we have deductive reasoning (of which he is such a master) at its best. But how the style would have suffered if he had recollected that "transmission of acquirements" and "characters which are inborn" were wholly meaningless expressions, and had had to adopt some laboriously constructed paraphrases in their stead. I should like, if Dr. Reid will allow me, to submit to his notice the following quotation from a work for which I have the greatest admiration :—

"Nevertheless, since these terms, 'innate,' 'acquired,' and 'inheritable' are firmly established in the literature of heredity, their use is now in many ways convenient. For the sake of clear thinking, it is necessary, however, to bear these real meanings carefully in mind. . . . We shall thus avoid the confusion of thought which is the common accompaniment of misleading terminology." *

And when he has read, marked, learned, and inwardly digested these golden words of wisdom, he may perhaps realise that he has been unwittingly straying into the regions that, for over a score of centuries, have been under the sway of The Logic of Classification ; in which case it may be confidently predicted that he will gracefully conform to "the laws of the country." †

* *The Laws of Heredity*, by Dr. Archdall Reid, chapter I., p. 19.

† After Dr. Reid had received a rough copy of this paper several letters were exchanged between us. In one of his, dated February 26th, 1914, he wrote as follows : " You are of opinion that 'inborn,' 'acquired,' 'inherited,' are class-names, and therefore when a man says, for instance, that inborn traits, but not acquirements are inherited he does not mean what he would mean did he use these words in their dictionary meanings. I cannot suppose you have no warrant for your opinions and your statement. Therefore you are in a position to state the meanings that biologists in general give these words ; or you are in a position to quote chapter and verse showing that individual biologists or schools of them have clearly indicated their meanings (e.g., when they say that one character is inborn and inherited and another not so, or when they say one character but not another has its representative in the germ-plasm). In science no one's opinions are of value ; you are bound, therefore, to quote your data. If you don't, your opponent has the right to state that he asked you, and that you would not. Of course, I shall do that if your paper is published in its present form." To this I replied on March 1st :

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you?" This has been inserted that the reader may judge for
what was demanded and how adequately or inadequately that
was met.

DR. REID ON "BIOLOGICAL TERMS"

gratulation that Dr. Reid's attempt to upset that equilibrium was based on a misapprehension.*

But, besides exhibiting a temporary forgetfulness of the logic of classification, Dr. Reid's article betrays throughout the underlying assumption that the question of terminology is of paramount importance to the student of heredity to-day. Nothing, I believe, could be further from the truth. People know too much nowadays of what happens between the fertilisation of a human ovum and the interment of the resulting centenarian to be misled by mere terms. They can follow in imagination the interplay of the ever-changing organism and its surroundings with a certitude that no terminological incongruity can ever shake. Laboriously thought-out abstract terms will never enable them to realise with greater vividness than they do that sex-cells and, consequently, babies differ; that how a child grows up depends on how it is reared; that muscular development is affected by muscular exertion; that a maimed parent cannot voluntarily beget a similarly maimed baby; and, lastly, that no amount of prenuptial Swedish exercises will ever help a man to beget a Samson. No, what our statesmen, our educationists, and our intelligent fellow citizens want biologists to do for them is to find out how much a human being's mental and physical traits are due to parentage and how much to rearing and education. And if, and when, such discoveries are made, there will be, we may feel absolutely certain, no especial terminological difficulty in making them understood. Nor would that difficulty in any way be diminished if Dr. Reid's suggested improvements were universally adopted. Let us consider, for example, the one which, if it had been made half a century ago, would, according to Dr. Reid, have saved Wallace,

old meanings had entirely ceased—blastogenic, somatogenic, etc., bear witness against that—but the real happenings (the facts) had become so well known that, whatever the terms employed, they could not fail to be intelligible.

* For classificatory purposes a word may serve (i.) as class-name (as thus used it has, technically speaking, no meaning); (ii.) as class-definition (as such it must be regarded as the sole *source* of meaning); (iii.) as class-indicator, or, as would, perhaps, be more accurate, class-definition-indicator. Incapable, through lack of the essential descriptive elements, of serving as the sole source of meaning (i.e., of standing for the class-definition), nevertheless, what significative reference it still possesses renders it more or less useful as an aid to the memory. To mistake a class-indicator for a class-definition is apt to lead to trouble.

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many others. Their age-long usage has laden them

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immemorial drama of life and its perennial renewal. And it is just this wealth of association that so fits the words "inborn" and "innate" for the expounding of heredity. It enables them, as they say in Yankeedom, to get right there at once. And what a burden to the flesh it is to have to turn from them to such abstractions *in vacuo* as "potentialities" and "stimuli"! People want to deal with something tangible or perceptible, such as the shape of the human head or the head itself. There is no danger, as Dr. Reid seems to fancy, of anyone thinking that a head could come into existence without *both* nutriment and germ-plasm, or a scar without physiological activity as well as external injury! Everyone knows, too, that if the owner of a head has offspring that offspring *must* have a head also, though not necessarily a scar. And so, the one being inevitable and the other not, what is there unreasonable in giving them different labels, say *x* and *y*, or inborn and acquired; it matters not intrinsically which.

But let us return in imagination, for a moment, to those Aryan progenitors of ours to whom we owe the hoary roots so lately quoted. There is one thing we may be quite certain about them. They were not embryologists. Life for them commenced at birth, and the biological characters then revealed would, without question, be overwhelmingly impressive. This being so, what so natural as that they should class them collectively under some such class-definition and class-name combined as "born-with." But in time we can fancy somebody saying that as *all* babies, if they live, have two sets of teeth, there *must* have been something in the babes at birth to predetermine those teeth, and urging that these and similar belated characters should be classed as "born-with" also. Doing this would, of course, be stretching the literal meaning of the term, and rob it of some of its fitness as a class-definition. But what would that matter? The actual facts would be far too familiar for anyone to be misled. And then a further stretching of its accepted meaning, and a further unfitting of "born-with" to serve as a class-definition would surely happen. For, sooner or later, it would certainly be claimed that other characters, such as the shape of a man's head or the quality of a child's memory, were also foreshadowed in the babe. And so, at last, a head, a beard, the shape of a nose and a mental aptitude would have all come to be classed as "born-with," *though*

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the stimulus of"—a fact that probably accounts for
not having been at once adopted by general
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* This is nothing compared to Herbert Spencer being made
and acquired into believing nothing could be evoked out of
something out of nothing.

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For plain people can understand "the stimulus of hunger," but not, unless they are already hungry, "the stimulus of nutriment."

Finally, I will try to state succinctly the governing ideas that prompted the writing of this paper. They were (i.) The conviction that, next to finding out what the order of Nature is, the scientist's most important task is to make that order understood *with the smallest possible demand on the attention and mental energy of those he is enlightening* ; * (ii.) That all terminologies must be judged by this standard ; (iii.) That abstract terms that can be so shuffled as to make it appear that Herbert Spencer *must* have thought that growth could proceed without food, or a man be scarred after death, must, as measured by the above standard, be worse than useless ; (iv.) That with regard to heredity, where the facts are so well known, the traditional terminology, anchored in such multitudinous ways to the concrete as it is, is practically all that is wanted, and anyhow—which is more to the point still—the only one that is practicable ; (v.) That the old familiar terms, when interpreted according to their context, have never misled anyone ; and (vi.) That, if the logomachists and verbalists would cease their fussing, there would be no more trouble.

If we could construct an entirely new terminology out of entirely new materials, and get it universally accepted, it would be a different matter. But this is out of the question, and mere tinkering, and especially tinkering with abstractions, can only cause confusion. But how came a writer of Dr. Reid's calibre to go a tinkering in this fashion ? It is clearly only another instance of the hypnotic influence of a favourite abstract phrase : the culprit on this occasion being "nature and nurture" (potentiality and stimulus). The truth that every character is a product of nature *and* nurture ; and that, consequently, no character can be a product of one or the other only, must have been known *in the concrete* even to the original owner of the Neanderthal skull ! But Dr. Reid, thinking only in abstractions, assumed (as seen above) that inborn and acquired had been, and were, regarded as identical with "product of nature" and "product of nurture" ; and then proceeded to build on this amazing premiss with all the ruthess logic of a Jules Verne. This, too, in spite of its involving the utter idiocy of Wallace, Spencer, Romanes and Weismann !

* See Herbert Spencer's Essay on "Style."

IN

TERMINOLOGY EXACTITUDE

By G. Archd.

THE Hermit
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The Hermit precise, descriptive, dictionary meanings
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denotations, and used in a special jargon as class labels,
nothing to supply their places!! "The science of heredity
deals (unlike theology and metaphysics) with the tangibl
perceptible, is not likely to have its ideas of actuality dislocat
terminology"!!! I felt exactly as if, in an argument concer

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the heavenly bodies, I had been told that the keywords of astronomy—flat, round, ellipse, orbit, rotation, revolution, and the like—had no relation to their dictionary meanings, but were just arbitrary class-names used with no fixed meanings.

However, if the Hermit is right, it should be easy for him to indicate the classes of characters which have been labelled by the words in dispute and to quote instances. I asked him to do so. Thereupon followed a correspondence in which, I think, our minds never really came into touch. Each of us was dominated by his own idea. Reading biological works, I could not believe that the writers had used their words in any but the ordinary way. The Hermit, bearing in mind the obvious fact that all characters are products of nature and nurture, of germinal potentiality and stimulus from the environment, could not believe that thinkers, great, famous, and much-to-be-revered, had neglected to bear it in mind also when naming some characters inborn and some acquired. Obsessed by my notion, I pressed him continually. "Tell me the meanings you suppose these words to have. Tell me the biologists who have so used them. Tell me what sense there is in their arguments when the words have been so used. However used, I will show they lead to absurd results if they be applied to characters." I got some lectures on the folly of supposing that great men can have been so blind as I supposed, and some most enlightening dissertations on the development of language. Eventually the Hermit added the note that appears at the end of his article. It will be noted that he names only me. I cannot do better than give the context of the passage he quotes.

"The names we use do not greatly matter if they serve to indicate the truth; an erroneous term, provided it is obviously erroneous, may convey a right impression. But the words inborn, acquired, and inheritable appear—when we think of individuals instead of the germ-plasm, as, very naturally, we tend to do—so obviously correct that their use has been, and is still productive of endless confusion and controversy. For example, it was formerly believed that parental acquirements were transmissible to offspring: in other words, it was maintained in effect that a character (*e.g.*, a scar) which the parent was able to acquire in a certain way (as a reaction to injury) because a long course of evolution had rendered such acquisition possible to the members of his race, tended to be reproduced by the child in a different

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circumlocution, I had grouped all stimuli under the class-name "nutriment." I had that I used the word only as a class-name for number of heat, cold, light, internal secretions, and the like. In number of BEDROCK I indicated that I thought it nutriment by itself was ever a stimulus. It is convenient with my use of the word "stimulus," to which objection though not by the Hermit. I am not the first to so use from a passage from Weismann quoted on a later page employ it merely to indicate those influences that cause change which convert germinal potentiality into actuality—injury of ductless glands, and what not. I am not wedded to the is the most suitable I know. The reader, if he preserves substitute for it any other he prefers.

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“ Nevertheless, since these terms innate, acquired, and inheritable are firmly established in the literature of heredity, their use is now in many ways convenient. For the sake of clear thinking it is necessary, however, to bear their real meanings carefully in mind—to remember always, when speaking of multicellular organisms, that by an inborn character is meant one which develops under the stimulus of nutriment, by an acquirement, one which develops under the stimulus of use or injury, and by inheritance the reproduction, as a nutritional character, by the child of a parental character of any sort. We shall thus avoid that confusion of thought which is the common accompaniment of misleading terminology. ‘ Men believe that their reason rules over words ; but it is also true that words react and, in their turn, use their influence on the intellect.’

“ Though the terms innate and acquired are misleading when used to compare or contrast the characters (which have developed under different stimuli) of the same individual, they are quite accurate when used to compare the characters of different individuals. Thus, when we say that an individual is innately like or unlike another, we imply, in effect, that the likeness or unlikeness is due to a germinal similarity or dissimilarity. On the other hand, when we say that the individuals agree or differ in their acquirements, we imply that the stimuli under which they developed have been similar or dissimilar in kind or degree. In the latter case the individuals, as compared to each other, have eaten, more or less, of different kinds of food ; taken more, or less, of different kinds of exercise, or received more, or less, of different kinds of injury.” *

Again I pressed the Hermit, warning him that, as time was getting short, I might have to quote from our private correspondence. As a sufficient reply he indicated the following from Weismann :—

“ By acquired characters I mean those which are not preformed in the germ, but which arise only through special influences affecting the body or individual parts of it. They are due to the reaction of these parts to any external influence apart from the necessary conditions for development. I have called them ‘ somatogenic ’ characters, because they are produced by the reaction of the body or soma, and I contrast them with the ‘ blastogenic ’ characters of an individual, or those which originate solely in the primary constituents of the germ.” †

Yet again I pressed the Hermit, saying Weismann appeared to me to use his words with their dictionary connotations. “ What do

* *The Laws of Heredity*, pp. 18—20.

† *The Germ-Plasm*. Eng. Trans. P. 392.

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Tell me precisely what he means when he says that the germ-plasm (as described above) do not affect the germ-plasm. "Define his meaning, as you suppose, of inborn (acquired (somatogenic), and inherited." "By inborn he meant those characters that must arise under the nutriment. He had not, I believe, hit on so completely but that is the essence of what he had in his mind. I have already dealt with. By inherit, I believe, he bodily resemblances resulting from the similarity of the germ-plasm to the grand-parental germ-plasm."

After this I pressed the Hermit no more. I knew for now that his case or mine was past praying for. I do not now that the reader is able to make of his statement; but what I is that Weismann maintains, (1) that like tends to beget like

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parent and child have been reared under like conditions, and (2) that some characters (*e.g.*, heads) arise inevitably because the stimuli in response to which they develop are always present; whereas others (*e.g.*, scars) do not develop inevitably because their appropriate stimuli are not always present, whence it follows that, if the father develops an inevitable character, the son tends to develop it also, whereas, if the father develops a non-inevitable character the son does not necessarily do so. It would seem that Weismann, if a profound, is also a somewhat obscure writer.

However, I cannot help thinking that the Hermit is mistaken—that he has been grievously misled by the very terminology he defends with so much eloquence. Formerly all divergencies of child from parent were called “variations.” It was more or less clearly recognised that some “variations” were innate or natural, and others acquired; but, since the parts of the child were supposed to be derived from the similar parts of the parent (the child’s head from the parent’s head, etc.), it was thought that both kinds tended to be transmitted. Lamarck, struck, doubtless, by the fact that living beings were adaptional forms and that “functional variations” were usually adaptive, attributed evolution to the effects of use and disuse. Next, Darwin, while not denying the Lamarckian principle, attributed evolution to innate variations. Next Weismann denied the transmission of acquired variations—not only the effects of use and disuse, but all others. Next Lloyd Morgan, Baldwin, and Osborn proposed, in view of the confusion that was arising, to limit the word variation to germinal, and the word modification to acquired differences, a suggestion that was generally adopted. All through this evolution of language—which should delight the heart of the Hermit—increasing accuracy of thought was accompanied by increasing precision in the use of words. In the passage quoted by him, Weismann, employing a terminology a little different from that which we now commonly use, is merely insisting on the difference between inborn and acquired “variations.”

Weismann’s merit is enormously greater than his defender apparently supposes. He is never obscure. He never utters truisms. He did not, of course, perceive the whole truth; no man ever does. He did not altogether separate the truth he had dug up from its accretions of error; that also is impossible to the pioneer.

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of the blacksmith develop from those of the normal man in re

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to use, those of the normal man from those of the youth in response to the same stimulus, and so on. Why, then, were normal traits regarded as inborn, and additions or subtractions from them as acquired? Obviously because the normal, like the variation, was not thought of as a product of functional activity. It was conceived as germinal—germinal as distinguished from somatic.* Consequently the fact that the development of the higher animals after birth is mainly a product of functional activity, and the fact that the principal feature of the rise of the higher animals was the evolution of the power of making use-acquirements, were missed. It was not till the closing years of the last century that attention was called to them. All this is historical. Yet the Hermit, shocked that great men should be accused of ignoring a truth so simple and obvious as that all characters are equally products of nature and nurture, indignantly declares that they must have used their terms as class labels.

I could have presented the Hermit with passages in which Weismann seems, more clearly than in the one already quoted, to use his words as class-names.

“In the meantime I should wish to point out that we ought, above all, to be clear as to what we really mean by the expression acquired character. An organism cannot acquire anything unless it already possesses the predisposition to acquire it; acquired characters are, therefore, no more than local, or sometimes general, variations which arise under the stimulus provided by certain external influences.”†

In this passage Weismann appears to distinguish such departures from the normal as are caused by *certain* external influences by the class name acquired. I presume he means use, disuse, and injury. In the following he applies it especially to characters that arise in response to use and disuse.

“Lamarck . . . laid special emphasis on the increased or diminished use of the parts of the body, assuming that the strengthening or weakening that takes place from this cause during the individual life, could be handed on to offspring, and thus be intensified and be raised to the rank of a specific character. Darwin also regarded this *Lamarckian principle*, as it is now generally called, as a factor in Evolution, but he was not fully

* See the passage from Weismann quoted in the present article (p. 121).

† *Weismann on Heredity*. Eng. Trans. Vol. I., p. 171.

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* Darwin and Modern Science, pp. 21—2.
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instance, I believe that scars and the effects of use and disuse are sometimes, and to some extent, transmissible."

B. (a neo-Darwinian): "I am sure they are not. The weight of evidence is altogether against that notion."

Z. (a heretic): "These arguments have been going on for fifty, and, for all I know, fifty thousands of years. I suppose there is some reason, which should be discoverable, why they never end. My difficulty is to know what is meant when it is said that such characters as heads are inborn and transmissible while such characters as scars and use-acquirements are less, or not at all, transmissible."

A. "What is your difficulty?"

Z. "I can understand when it is said that a variation is inborn and transmissible. I can even understand, though much less clearly, when I am told that a modification becomes inborn and transmissible. The talk is then about the relations of likeness and difference between individuals, and you are maintaining that similar relations will be perpetuated. In the former case you mean that there is a germinal difference between parent and child (or between two other individuals), and that descendants tend to resemble the child. In the latter case you mean that there is an acquired difference between parent and child, and that this difference tends to become germinal in descendants and so to be handed on. But when you contrast not likenesses and differences between individuals, but the characters of the same individual, and call such things as heads 'inborn,' 'germinal,' and 'transmissible,' and such things as scars 'acquired,' 'somatic,' and 'non-transmissible,' I cannot conceive what you mean. In the germ-cell, through which all inheritance occurs, there is neither head nor scar, nor any of the effects of use, but only the potentiality of producing them in response to fitting stimuli. In what sense, then, is a head more inborn and less acquired than a scar. Both are derived from the germ-plasm and both develop in, and are parts of, the soma. Obviously, therefore, they are both inborn, acquired, and inheritable in exactly the same sense and degree—the inborn element being supplied by the germinal potentiality and the acquired element by the stimulus. What, then, do you mean by saying that the scar tends to become inborn and inheritable? It is already as inborn and inheritable as

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occurred, that modification tended to produce a corresponding germinal change."

Z. "Haven't you changed your meanings again? What kind of germinal change? What is the effect of it?"

B. "I think, perhaps, A has not expressed himself very happily. It is not so much normal traits that are termed acquired as those that result from the exercise of function. At any rate, that is what I had in mind when arguing against the transmission of acquirements."

Z. "You mean the effects of use and disuse. Apparently you do not include the effects of injury. Others have included it; however, that does not signify. In any case, you are using the words 'inborn,' 'acquired,' 'blastogenic,' 'somatogenic,' and the like, as class-labels, with meanings quite different from their dictionary meanings, and quite different from the meanings you give them when you talk of inborn and acquired likenesses and differences—of, for instance, variations and modifications. Why not say the thing you mean in plain language? Besides, what does A mean when he says that acquired as well as inborn characters tend to be inherited, and what do you mean when you contradict him? Does he mean that a character that develops under the stimulus of use in the parent tends to develop, under some other stimulus, in the child? Is that what you dispute?"

B. "Yes."

Z. "Then, when you talk of an inborn trait as being 'transmissible' your word does not mean the same as when you use it in reference to an acquirement. If you say that a child inherits an inborn character you mean, and can only mean, that the child is *like* the parent in that it tends to produce the same kind of character under the same stimulus; but, if you say that a child inherits an acquirement, you mean apparently that the child is so profoundly *unlike* the parent that it produces the character under totally different conditions. It seems, therefore, that you not only use 'inborn' and 'acquired' as class-labels, but also 'inherit,' and that you use all these words with their diverse meanings in the most confusing juxtaposition—sometimes even in the same sentence."

B. "No, not confusing. No confusion has ever occurred. It

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may be difficult to define the words, and they may mean different things at different times. But no harm has followed. Every man's meaning has always been clear to his fellows."

Z. "Do you say, in all seriousness, that such men as Lamarck, Spencer, and Romanes, consciously argued that characters, for example, scars and muscular developments, which evolution had fitted the species to develop in response to injury and use, and which past generations had so developed for millions of years, tend suddenly, in the individual that happens to come under observation, to develop in response to quite other stimuli and in a way with which evolution had nothing to do?"

B. "Yes, I suppose so."

Z. "And do you say that such men as Wallace and Weismann understood all this, and not only treated their opponents with respect, but never mentioned it?"

B. "I suppose so."

Z. "Were it not that I think you utterly wrong, I would begin to think that there was some warrant for the words used by a most charming, but most wrong-headed writer, the Hermit of Prague. 'Hopeless imbeciles' is what he said."

B. "Well, anyhow, as I say, no great harm was done. Notwithstanding our friend A, the Lamarckian theory is dead."

Z. "No, only half dead, and only after a century of controversy. And to this day other sects are still using, consciously or unconsciously—I think unconsciously—the same words in the same vague way and with the same multiple meanings. Some characters are still being called inborn and inheritable, and others acquired and non-inheritable. Have you any notion what the Mendel-Mutationist means when he declares that mutations, but not fluctuations, have their representatives in the germ-plasm? What do you suppose that Professor Karl Pearson thinks he is proving? Is it that like tends to beget like when parent and child develop under like conditions? Is it that like tends to beget like even when parent and child develop under unlike conditions? Is it that moral and intellectual traits develop under the same influences as physical traits—for instance, that morality and modesty, like stature and span, develop under the influence of physical exercise, so that the more, within limits, a child plays in the fresh air (and, as a

TERMINOLOGICAL INEXACTITUDES

consequence, eats) the more moral and intellectual it becomes. Can you tell me in plain language exactly what he is trying to prove ? ”

B. “ Don’t ask me. I am neither a Mendelian nor biometrician.”

Z. “ Mendelians and biometricians touch only a very small part of the total field and are very modern. But discussions about heredity have been going on for a very long time. Presumably students of living beings have an average amount of intelligence, and are as capable as other people of perceiving truth and reaching agreement. Can you name a single biological controversy of importance that has ever ended, or a single interpretation that is universally accepted by biologists ? No other science is in such evil case. What, if not the use of equivocal and misleading terms, has been the cause of biological failure ? ”

B. “ The subject is a very difficult one. In biology it has not been possible to measure and test with the accuracy of physicists and chemists. But it is easy to talk. How would you proceed if you were asked if a given character were inheritable ? ”

Z. “ I should at once reply that it was certainly inheritable in the same sense and degree that every other character is inheritable. But probably you mean to ask what likelihood there would be of that character being reproduced by the child ? ”

B. “ Yes.”

Z. “ I should first try to find out under what stimulus that character developed—use, injury, internal secretions or what not, a purely physiological task. Next, I should try to evaluate the chances of that stimulus acting on the child. If it were an internal secretion it would be almost sure to act ; the child would produce that trait almost as certainly as his head. In many cases use, and in almost all cases, injury, would act with lesser degrees of certainty. I should at least have a clear idea of what I had to do, and how to set about it.”

B. “ Wouldn’t everyone else proceed in exactly the same way ? ”

Z. “ No. Judging by the past, every biological sect would ignore the part played by the environment. The problem would not be a physiological one for any of them. The Lamarckian and the Neo-Darwinian would set about arguing whether the character was inborn or acquired. The Mendelian would appeal to his ‘ new science of Genetics.’ He would cross individuals who had the

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character with individuals who had it not. If it obeyed the Mendelian laws he would tell you it was inborn with its representative in the germ-plasm, and that, accordingly as it was dominant or recessive, it would be inherited by a certain proportion of offspring. If it did not obey those laws he would tell you it was an acquirement 'due to conditions of the environment, to nutrition, correlation of organs, and the like,' and, therefore, unworthy the attention of 'modern and exact' science. The biometrician would ascertain whether its coefficient of correlation agreed with, or was higher than, or lower than, that of some other character which, for some reason, he regarded as undoubtedly 'bred,' and would then, after monumental labours and the lapse of years, tell you whether or not it was 'created.'"

B. "I daresay you think yourself wiser than everyone else. You seem to me a mere dialectician."

A. "A mere logomachist."

Exeunt A and B. Z orders for luncheon new potatoes and green peas, which, in dread of falling a victim to class names, he inspects very closely.

REVIEW

THE UNEXPURGATED CASE AGAINST WOMAN SUFFRAGE, by SIR
ALMROTH WRIGHT, F.R.S. (Constable & Co., Ltd.) Demy 8vo.
2s. 6d. net.

In the pages of the *Nineteenth Century* for June, 1889, appeared a Protest against Woman Suffrage signed by more than one hundred prominent ladies. The main argument in that appeal, briefly stated, was that inasmuch as the work women can do for the State and their responsibilities towards it must always differ essentially from those of men, therefore their share in the working of the State machinery should be different from that assigned to men. The development and administration of the chief national resources, the writers said, the diplomatic affairs of State, the conduct of Imperial defence, the performance of the most fundamental and laborious industries upon which our national life is built, the management of naval and mercantile shipping upon which depends our food supply—all these duties of necessity devolve exclusively upon men; while “women’s direct participation in them is made impossible either by disabilities of sex or by strong formations of custom resting ultimately upon physical differences against which it is useless to contend.” Much water has run under the bridge since 1889; but the essential physical differences between the sexes as set forth in the above extract remain unchanged. Upon men alone we still depend for our food supply, for the major portion of our commercial activities, and for the sound development of national and Imperial finance, defence and diplomacy. The oft-repeated demand of the Feminist that women can and should be economically independent—or, as Olive Schreiner expressed it, “we claim all labour for women”—is founded upon a fantastic delusion which can find no possible fulfilment so long as the physical sex disabilities of women exist. Upon these relative differences of function Sir Almroth Wright bases his case against Woman Suffrage. His views, as those of a distinguished scientific expert, command respectful attention, but much of the force of his arguments is marred by an unhappy choice of exaggerated and unnecessarily bitter expression. Few will disagree with his main proposition, that the primary point to be decided is whether the extension of the franchise to women is

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in the public interest. J. S. Mill, the Apostle of Suffragists, wrote in like terms in his essay on Liberty—"I regard utility as the ultimate appeal on all questions of ethics." The vote, says Sir Almroth, controls the rulership of the State, and State control is and must be carried on by physical force. Such force may be employed directly or held in suspension while represented by prestige. But the power of prestige depends upon the existence of force in the background, to which an ultimate appeal can be made, and with the disappearance of that possibility the efficacy of prestige vanishes. An electoral contest partakes of the nature of a civil war. The combatants by long custom have agreed to accept the decision of a count instead of actually breaking heads. Physical force is held in suspension, but nevertheless it remains as the supreme factor. In such a struggle woman cannot be regarded as a compelling force. The connection of woman with violence is a retrograde idea, an offence to the moral teaching of civilisation. Hence, whenever woman has recourse to violence she places herself in a false and immoral position; one which is intolerable to civilised man. To give the vote, therefore, to woman would destroy the power necessary to the stability of our electoral system, and consequently endanger the State. This view of the supremacy of physical force in government is shared by prominent Suffragists. "The true duty of Government," declared the British Constitutional Society (of which Sir W. Chance and Lord Hugh Cecil were members) in June, 1912, "is to protect us both from the enemy abroad and from the malefactor at home. The more that duty is overlaid and obscured the worse it will be performed." The resentment shown by many persons when allusion is made to the rule of force seems to indicate confusion of thought. The fact that physical force is the deciding factor in government involves no denial of the power of moral force. The two influences are not essentially antagonistic. The history of civilisation proves the reverse. Physical force may be misused as in the case of a drunken ruffian who beats his wife, or it may be used for a moral purpose as when that ruffian is overpowered by a spectator of his crime. In both instances the deciding factor is the same. But as Sir Almroth is careful to note, legislation is not brought about solely by the votes of electors. Another equally potent factor is a necessary precedent, namely, the preparation and guidance of public opinion. Effectual law is the crystallisation of public opinion, in the development of which woman's influence is equal to, if not greater than, that of man. The duty of the legislator towards the community demands continual compromise between conflicting interests, and individual cases of hardship and injustice are inevitable. Woman, by reason of those emotional sympathies developed by her inherent instincts of motherhood, does not accept compromise. As Sir Almroth Wright expresses it, "her instinctive morality is personal and domestic, not public." The alternative

REVIEW

before us then is plain. Either woman must eradicate from her nature those tendencies, the possession of which enable her to perform her most important function, namely, to nurture and tend the individual, or she remains an undesirable voter. Surely Sir Almroth's conclusion that the interests of the State are best served by diversity rather than by identity of function is in accordance with the opinion expressed by the memorialists above mentioned; who, in appealing "to the common sense of English men and women," wrote that "citizenship is not dependent upon or identical with the possession of the Suffrage. It lies in the participation of each individual in effort for the good of the community." It is indisputable that recent years have brought about a great congestion of business in Parliament. Legislation is pressed forward at an ever-increasing pace and many desirable measures remain shelved while many that are passed fail to achieve their object owing to want of detailed consideration. A remedy for these evils must be found. But the cure cannot be in a vast increase of the electorate by the addition of a sex whose physical conditions do not permit them to undertake the primary duties of government. On the contrary, the more numerous the electorate the greater the congestion of varying desires. A more reasonable course would be an enlargement of the powers of those organisations to whom is entrusted already the preliminary shaping of social and domestic legislation, namely, local councils. In this important and essential work the special abilities of women are of increasing value, and in this direction may be discovered the true solution of the Woman Suffrage controversy.

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